mented upon. The contractions of the heart were more frequent after alcohol during complete rest, from five to ten beats per minute for some time; and when exercise was taken the increase was greater. The mean pulse of the twenty-four hours was, however, not increased unless the amount of alcohol was large and repeated. In other words, the heart's beats were less frequent than natural when the effect of the alcohol had passed off. The pulse became both fuller and softer to the touch; and this relaxation of the radial artery was shown also by the sphygmograph. That the smaller vessels were relaxed, was shown both by the redness of the surface and by the evident ease with which the blood traversed the capillaries, as shown by the sphygmographic tracings.

6. The respirations were not increased in number by alcohol; they were rather lessened, and were deeper in some of the experiments; but the effect was not very marked.

III. "Experimental Demonstrations of the Stoppage of Sound by partial Reflections in a non-homogeneous Atmosphere." By JOHN TYNDALL, D.C.L., LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.

(See Paper read Jan. 15, *antè*.)

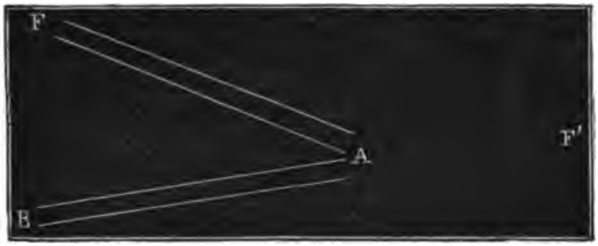
IV. "On the Division of a Sound-Wave by a Layer of Flame or heated Gas into a reflected and a transmitted Wave." By JOHN COTTRELL, Assistant in the Physical Laboratory of the Royal Institution. Communicated by Professor TYNDALL, F.R.S. Received February 2, 1874.

The incompetency of a sound-pulse to pass through non-homogeneous air having been experimentally demonstrated by Dr. Tyndall, and proved to be due to its successive partial reflections at the limiting surfaces of layers of air or vapour of different density, further experiments were conducted in order to render visible the action of the reflected sound-wave.

The most successful of the various methods contrived for this purpose consists of the following arrangement. A vibrating bell contained in a padded box was directed so as to send a sound-wave through a tin tube, B A (38 inches long, 1¾ inch diameter), in the direction B F', its action being rendered manifest by its causing a sensitive flame placed at F' to become violently agitated.

The invisible heated layer immediately above the luminous portion of an ignited coal-gas flame issuing from an ordinary bat's-wing burner

was allowed to stream upwards across the end of the tin tube B A at A. A portion of the sound-wave issuing from the tube was reflected at the limiting surfaces of the heated layer; and a part being transmitted through it, was now only competent to slightly agitate the sensitive flame at F'.



The heated layer was then placed at such an angle that the reflected portion of the sound-wave was sent through a second tin tube, A F (of the same dimensions as B A), its action being rendered visible by its causing a second sensitive flame placed at the end of the tube at F to become violently affected. This action continued so long as the heated layer intervened; but upon its withdrawal the sensitive flame placed at F', receiving the whole of the direct pulse, became again violently agitated, and at the same moment the sensitive flame at F, ceasing to be affected, resumed its former tranquillity.

Exactly the same action takes place when the luminous portion of a gas-flame is made the reflecting layer; but in the experiments above described, the invisible layer above the flame only was used. By proper adjustment of the pressure of the gas, the flame at F' can be rendered so moderately sensitive to the direct sound-wave, that the portion transmitted through the reflecting layer shall be incompetent to affect the flame. Then by the introduction and withdrawal of the bat's-wing flame the two sensitive flames can be rendered alternately quiescent and strongly agitated.

An illustration is here afforded of the perfect analogy between light and sound; for if a beam of light be projected from B to F', and a plate of glass be introduced at A, in the exact position of the reflecting layer of gas, the beam will be divided, and one portion will be reflected in the direction A F, and the other portion transmitted through the glass in the direction F', exactly as the sound-wave is divided into a reflected and transmitted portion by the layer of heated gas or flame.

February 19, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the Table, and thanks ordered for them.

The following Papers were read:—

I. "On the Absorption of Carbonic Acid by Saline Solutions." By J. Y. Buchanan, Chemist on board H.M.S. 'Challenger.' Communicated by Prof. Williamson, For. Sec. R.S. Received December 11, 1873.

(Abstract.)

Until lately it was believed that the atmospheric gases dissolved in sea-water could be extracted from it, as from fresh water, by boiling *in vacuo*. The merit of the discovery that such is not the case is due to Dr. Jacobsen, of Kiel, who found that, in order to drive out the whole of the carbonic acid, the water must be evaporated almost to dryness, and that no amount of boiling *in vacuo* will suffice to eliminate it. Being particularly interested in the matter, I immediately commenced a series of experiments to determine, if possible, the salt or salts to which sea-water owes this property.

Preliminary observations satisfied me, in the first place, that sea-water has this property, and, secondly, that solutions of the sulphates of magnesia and of lime possess the same property. In order to gain more precise information, two series of experiments were made, the one analytical, the other synthetical. The former consisted in saturating saline solutions with carbonic acid, and then distilling them, the carbonic acid passing in the various fractions being determined; the latter, in determining the absorption coefficients of two solutions, the one of sulphate of magnesia, the other of sulphate of lime.

First, the analytical series.—Before proceeding to saline solutions, distilled water was saturated with carbonic acid and distilled. The first eighth of the distillate contained abundance, the second a trace, and the remainder no carbonic acid. It may therefore be assumed, in the experiments which follow, that the carbonic acid held *simply in solution* by the water passes almost entirely in the first eighth of the distillate, and that whatever passes afterwards has been retained, in some way or other, by the salt in solution.

Experiments were made on solutions of sulphate of magnesia, of sulphate of magnesia and chloride of sodium, and of sulphate of lime, to which were added some on sea-water itself. In every experiment the quantity of solution operated on was 300 cub. centims., which was boiled in a flask connected by a doubly bored cork with a Liebig's condenser, which was fitted at its other end, air-tight, into a tubulated

receiver. To the tubulure was attached a bulbed U-tube, and, by means of an aspirator, air could be constantly sucked through. The carbonic acid coming off was retained by baryta-water of known strength distributed between the receiver and the U-tube; what remained unneutralized was determined by oxalic acid, the point of neutralization being indicated by rosolic acid. The oxalic acid was rather stronger than tenth normal; it contained 6·478 grammes $C_2H_2O_4+2H_2O$ in the litre. One litre baryta-water required 3235 cub. centims. oxalic acid for neutralization.

The method of conducting the operation was as follows:—Carbonic acid was passed through the solution until it could be assumed to be saturated. The object being to determine the carbonic acid retained by the salt, it was necessary to get rid, as much as possible, of the simply dissolved gas. This was effected by drawing six or seven litres of air through the solution cold, then heating it to boiling, and allowing it to boil for a couple of minutes in a current of air. The receiver, with the baryta solution, was then attached, and the distillation continued in a current of air, until the contents of the flask were nearly dry. The amount of carbonic acid was given by the remaining alkalinity of the baryta-water.

Experiments on sulphate-of-magnesia solution, containing 12·3 grammes crystallized salt per litre.—As all were conducted in precisely the same way, it will be sufficient to give the results in a tabular form. The first three experiments were made with one and the same solution; for the last two a fresh solution, prepared, to all appearance, in exactly the same way as the previous one, was used. The difference in the results shows the precarious nature of the combination.

Volume of solution used.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 cub. centims.	Grammes carbonic acid in one litre.
cub. centims.	cub. centims.	cub. centims.		
300	25	78·96	0·0043	0·0143
300	10	30·00	0·0053	0·0165
300	10	30·90	0·0033	0·0110
300	15	47·50	0·0023	0·0077
300	10	31·32	0·0023	0·0077

Two experiments were made with a solution prepared as follows:—The quantity of sulphuric acid necessary for the formation of 12·3 grammes crystallized sulphate of magnesia was diluted to a litre, and pulverized carbonate of magnesia suspended in it. Although the mixture was allowed to stand over night, shut off from the influence of the atmosphere, the solution was still very acid. It is well known that carbonate of magnesia is difficultly soluble in cold dilute acids. To have heated the solution would have frustrated the object of the experiment, which was, by bringing nascent sulphate of magnesia together with

nascent carbonic acid at ordinary temperatures, to give them the best opportunity of combining. Two experiments were made with a similarly prepared solution of sulphate of lime. In this case sulphuric acid was added to the water in quantity sufficient to form, with lime, more salt than would dissolve in the liquid. Here neutralization took place without difficulty; and, as might have been expected, the amount of carbonic acid found was considerably greater than in the case of the magnesia salt.

Two experiments were made with an ordinary sulphate-of-magnesia solution, containing 2·05 grammes crystallized salt per litre.

Two further experiments were made with a solution containing 2·05 grammes sulphate of magnesia and 20 grammes chloride of sodium per litre. All were conducted in the way described above, and the results are given in the following Table. The experiments with the carbonates of magnesia and of lime were made at a considerably later date than the others; the value of 10 cub. centims. baryta-water had in consequence become equivalent to 32·0 cub. centims. instead of 32·34 cub. centims. oxalic acid:—

Nature of solution.	Volume of solution.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 cub. centims.	Grammes carbonic acid in one litre.
	cub. centims.	cub. centims.	cub. centims.		
$MgCO_3, H_2SO_4$	300	10	30·6	0·0032	0·0107
	300	10	30·9	0·0025	0·0083
$CaCO_3, H_2SO_4$	300	10	27·5	0·1014	0·3380
	300	10	27·5	0·1014	0·3380
2·05 grms. $MgSO_4 + 7H_2O$ per litre......	300	10	31·2	0·0026	0·0087
	300	10	31·3	0·0023	0·0077
$MgSO_4\ 7H_2O + NaCl$	300	10	31·6	0·0016	0·0053
	300	10	31·4	0·0021	0·0070

Five experiments were made with sea-water taken at the end of Portobello Pier, on the Firth of Forth. In the first three it was submitted immediately to the same treatment as the saline solutions; in the last two carbonic acid was first passed through it for some time. As the results are identical, it is evident that, in its natural state, the water in question was practically saturated with carbonic acid in this peculiar state of combination.

Volume of sea-water.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 cub. centims.	Grammes carbonic acid in one litre.
cub. centims.	cub. centims.	cub. centims.		
300	15	39·75	0·0198	0·0660
300	10	23·00	0·0211	0·0703
330	10	23·15	0·0208	0·0693
300	10	23·34	0·0203	0·0677
300	10	23·34	0·0203	0·0677

From the large amount of organic matter poured into the Forth in the neighbourhood of Portobello, there must be an abundant production of carbonic acid in the water itself; and we have seen above the effect of bringing it together in the nascent state with sulphate of lime. Sea-water contains on an average about 8 parts sulphate of lime in 10,000. A saturated solution of the same salt in distilled water contains, at 15° C., 24 parts in 10,000. Under the most favourable circumstances, then, sea-water might be expected to bind about one third of the quantity retained by an equal volume of saturated gypsum solution. We have seen that a litre of this solution is capable of retaining 0·338 grm. CO_2, while the same volume of sea-water contained at the most only 0·07 grm., or very little more than one fifth of that held by the sulphate of lime.

In ocean-water I have never yet found more than 0·064 grm. CO_2 per litre, including both *simply dissolved* and *half bound*. We have, then, in the sulphate of lime alone an agent capable of retaining much more carbonic acid than is usually found to exist in sea-water; and there is besides the sulphate of magnesia; so that whatever may be the function of the other salts, we do not, in order to find a *vera causa* for the phenomenon under consideration, require to go beyond the sulphates; and the practical lesson to be learned is that, if we get quit of the sulphates, the carbonic acid will be more easily disengaged by heat.

This is entirely borne out by experiment. In determining the carbonic acid in sea-water, I always add to it a sufficient quantity of a saturated solution of chloride of barium; and I find that, after about the first fifth of the distillate has passed, there is rarely a perceptible turbidity in fresh baryta-water.

The synthetical experiment consisted in determining directly the coefficients of absorption of a 1·23 per cent. solution of crystallized sulphate of magnesia and of a 0·205 per cent. solution of $Ca\,SO_4 + 2H_2O$. In Table I. the results of experiments on the magnesia solution are given, where the observations were made without loss of time. In Table II. the results of experiments on the same solution are given, only here the duration of the reaction was taken into account. The first reading was made at the highest pressure after the gas and solution had been together for nine days; the pressure was then successively reduced, and the other readings made at intervals of twenty-two, forty-one, and twenty-five hours from each other, the last of all being made only after the lapse of some days. Table III. gives the results of experiments on the gypsum solution, the readings in this case being made without allowing much time for the reaction to take place.

Table I.

Pressure in millims.	476·54	652·7	581·14	736·73
Temperature C.	12·0	11·5	11·9	11·9
Absorption coefficient of $Mg\,SO_4$ solution	0·7095	0·9562	0·8496	1·0545
Absorption coefficient of water	0·6909	0·9631	0·8455	1·0718

TABLE II.

Pressure in millims.	832·7	695·3	551·5	498·1	468·6
Temperature C.	11·1	11·0	10·45	11·1	11·1
Absorption coefficient of $Mg\,SO_4$ solution	1·2467	0·9331	0·8823	0·8974	0·8221
Absorption coefficient of water	1·3052	1·0445	0·8461	0·7546	0·7014

TABLE III.

Pressure in millims.	554·9	683·8	765·3	770·8	805·2	869·5
Temperature C.	10·1	12·9	13·3	11·1	11·1	11·65
Absorption coefficient of $Ca\,SO_4$ solution	0·8845	0·9923	1·0651	1·1885	1·2191	1·2964
Absorption coefficient of water	0·8617	0·9618	1·0624	1·1534	1·2048	1·2757

The general result of these experiments is, that sulphate-of-lime solution absorbs a little more carbonic acid than water, but follows the same law of variation with temperature and pressure; sulphate-of-magnesia solution differs slightly from water when but little time is left for the reaction to complete itself. If, however, the gas and solution are left in contact for a considerable time, the difference between the coefficients of water and of the salt solution becomes very marked, that of the latter being less for high pressures and greater for low ones than that of water.

The details of these experiments will be found elsewhere in a more extended paper.

II. "On an Instrument for the Composition of two Harmonic Curves." By A. E. DONKIN, M.A., F.R.A.S., Fellow of Exeter College, Oxford. Communicated by W. SPOTTISWOODE, Treas. R.S. Received November 6, 1873.

The interest in such compound curves lies in the fact that as a simple harmonic curve may be considered to be the curve of pressure on the tympanic membrane when the ear is in the neighbourhood of a vibrating body producing a simple tone, so a curve compounded of two such simple harmonic curves will be the curve of pressure for the consonance of the two tones which they severally represent, and thus the effect on the ear of different consonances can be distinctly represented to the eye.

If the motion of a point be compounded of rectilinear harmonic vibrations and of uniform motion in a straight line at right angles to the direction of those vibrations, the point will describe a simple harmonic curve.

Thus a pencil-point performing such vibrations upon a sheet of paper moving uniformly at right angles to their direction would draw such a curve.

The same kind of curve would also be drawn by keeping the pencil fixed and by giving to the paper, in addition to its continuous transverse motion, a vibratory motion similar and parallel to that which the pencil had; and if the motion of the latter be now restored, a complicated curve will be produced whose form will depend on the ratio of the numbers of

vibrations in a given time of the pencil and paper, and which will be the curve of pressure for the interval corresponding to this ratio.

The manner in which these three motions are combined in the machine is as follows:—Two vertical spindles, A and B, revolving in a horizontal

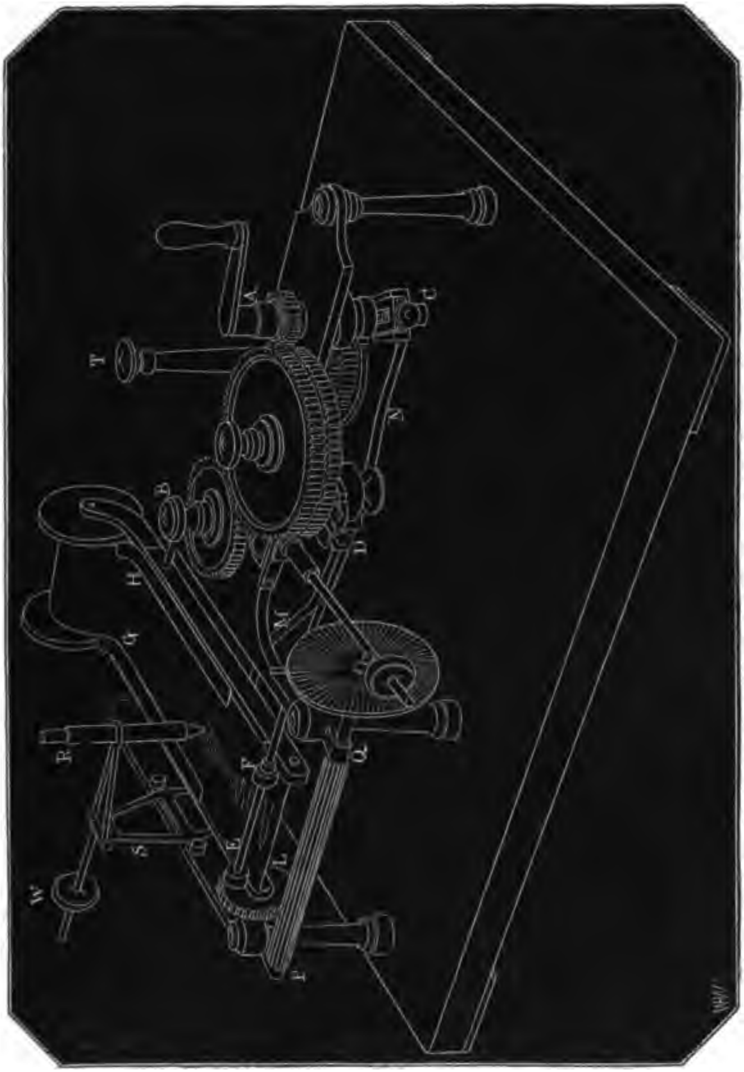


plate carry at their lower ends each a crank, C and D, and at their upper ends each a wheel cut with a certain number of teeth; these two wheels can be connected by means of an intermediate one, as is seen in the figure; and since either wheel of the pair can be replaced by another with a different

number of teeth, the relative angular velocities of the spindles can be regulated at pleasure. The paper upon which the curve is to be drawn is carried upon a rectangular frame, E F G H, capable of sliding horizontally up and down in a direction parallel to that of the plane passing through the spindles. This frame has a pair of rollers, E F and G H, at each end connected by tape-bands, between which the paper passes as the rollers turn. In order to give a motion of revolution to the rollers, a wheel, L, is fixed upon the axis of one of them whose teeth gear into those of a pinion, P Q, alongside which the frame slides, and which is itself driven by one of the vertical spindles. A connecting-rod, D M, is carried to the frame from the crank of this spindle, so that upon turning the latter a vibratory motion is given to the former; and since the transverse motion of the paper also depends upon the same spindle, a fixed pencil-point resting on it would draw a simple harmonic curve whose amplitude would depend on the radius of the crank, and wave-length on the transverse speed of the paper, which can be regulated at pleasure by means contrived for the purpose*.

A vibratory motion similar and parallel to that of the frame is given to a small tubular glass pen, R, so arranged as to move with its point lightly resting upon the paper. This motion is communicated by a connecting-rod, C N, from the other crank, which is carried underneath the sliding-frame and jointed to the lower end of a small vertical lever, S, to whose upper end the arm carrying the pen is attached.

The weight W serves to regulate the pressure of the pen on the paper, as it can be screwed in or out. T is merely a pillar upon which the change-wheels can be placed for convenience.

If the pair of wheels on the spindles are now connected by the intermediate one, it is plain that, upon turning either of the spindles by a winch provided for the purpose, the two motions of the paper will be combined with that of the pen, and the curve drawn will be that composed of the two simple harmonic ones, which would be the result of separately combining the harmonic vibrations due to each crank with the transverse motion of the paper. Thus if m and n are the numbers of teeth on the pair of wheels respectively, the equation to the resultant curve will be

$$y = \sin mx + \sin nx.$$

This equation implies that not only are the radii of the cranks the same, but also that they start parallel to each other and at right angles to the vertical plane passing through their axes: both these conditions can, however, be altered; and therefore the general form of equation to the curves which the machine can draw will be

$$y = a \sin (mx + \alpha) + b \sin (nx + \beta),$$

* It should be observed here that the vibratory motion thus given to the frame is not truly harmonic. In order to make it so, a more complicated contrivance than the simple crank and connecting-rod would have to be adopted; but this would probably introduce, through unavoidable play, an error greater than the present one, the length of the connecting-rods and the small size of the cranks rendering the latter nearly inappreciable. The motion will, however, for the sake of convenience, be considered truly harmonic throughout

where a and b are the radii of the cranks, and α and β are dependent on their relative inclinations to the above-mentioned vertical plane at starting.

As an example, suppose that $a=b$, while the ratio of m to n is as 2 to 1; then the above equation will represent the curve of pressure for the octave. Similarly, if m is to n as 16 to 15, the resultant curve represents the effect on the ear of a diatonic semitone, while the ratio 81 to 80 would give that of the comma. In both these curves, and more especially in the latter, the beats which would ensue on actually sounding the two tones together are shown with remarkable distinctness.

As the machine is provided with a set of change-wheels, many different curves can be produced, while the form of each can be more or less changed by altering the relative positions of the cranks before bringing the idle wheel into gear. It is also possible to obtain very large values of m and n in the above equation by using two idle wheels on the same axis which shall come into gear, the upper one with the wheel on the one spindle, the lower one with that on the other.

Thus, suppose A and B are the numbers of teeth on the spindle-wheels respectively, C and D those on the idle wheels, and let A gear with C and D with B; then $\frac{m}{n}=\frac{\mathrm{BC}}{\mathrm{AD}}$. Now, by properly choosing the four wheels, large values of m and n may be obtained. If, for instance, $\mathrm{A}=81$, $\mathrm{B}=80$, $\mathrm{C}=55$, and $\mathrm{D}=27$, $\frac{m}{n}=\frac{4400}{2187}$, this ratio being nearly $=\frac{2}{1}$, the corresponding curve will represent the effect of an octave slightly out of tune. The period of such curves as these being very long, it is necessary to have a good supply of paper; and this is arranged by carrying a reel-full on the horizontal frame, from which it is slowly unwound between the rollers. The rate at which this takes place has a good deal of influence on the form of the resultant curve; the slower it is the more compressed will the latter appear. Instead of using paper, the curves, provided the periods are short enough, may be drawn on slips of blackened glass, which can be carried along between the tapes connecting the rollers; they can be at once placed in a lantern and thrown on a screen.

The width of contour of any curve depends on the radii of the cranks; these may have any value between 0 and half an inch, and therefore the limit of possible width at any part will be two inches; so also, by altering the radii, a series of curves may be produced corresponding to the consonances of tones not of the same intensities. Since the maximum width of any curve will be double the sum of the radii of the cranks, the paper is cut to a width of two and a half inches, within which all curves which can possibly be drawn will be comprised.

The instrument is constructed by Messrs. Tisley and Spiller, of Brompton Road, to whom some improvement upon the original model is due.

III. "On the Number of Figures in the Period of the Reciprocal of every Prime Number below 20,000." By William Shanks. Communicated by the Rev. George Salmon. Received December 2, 1873*.

The following Table, in reality the joint production of the Rev. George Salmon, F.R.S., and myself, was commenced, and indeed nearly completed, before either calculator was distinctly aware that Burckhardt, Jacobi, or Desmarest had written or published any thing on the same subject. This fact is perhaps to be regretted; but it has led to the independent recalculation, by two different methods, both of Burckhardt's (Jacobi's Table is professedly a reprint of Burckhardt's) and of Desmarest's Table, and has resulted in the detection of several errors, which have, as far as I know, never before been pointed out. These errors, in the first place regarded as discrepancies, have been carefully examined; in fact every case has been reworked by me, with the view of either proving or disproving the accuracy of such numbers as differ from those in our Table. The result is, that such discrepancies are found to be errors both in Burckhardt and Desmarest. The two lists of errors are given below.

I now proceed to give the theorems used, and some account of the means employed by me in forming the Table.

Let P be any prime number, except 2 and 5. Then, from Fermat's theorem, we have $\frac{10^{P-1}}{P}\equiv 1$; or, adopting the usual notation, $10^{P-1}\equiv 1$.

Again, since the number of figures in the period of the reciprocal of all primes is not P$-$1 (or, in other words, since 10 is not a primitive root of all primes),

Let $10^{\frac{P-1}{n}}\equiv 1$, where n is even or odd, not less than 2, and not greater than $\frac{P-1}{2}$. Then we have

(1) The number of figures in the period of the reciprocal of P is either P$-$1 or a submultiple of P$-$1.

(2) Let a and b be integers, and let m be the remainder from $\frac{10^a}{P}$; that is, let $10^a\equiv m$; then $10^{ab}\equiv m^b$.

In practice b is never greater than 2, at least little or no advantage is gained by putting b higher. Also ab need not be greater than $\frac{P-1}{2}$.

Cor. When m is greater than $\frac{P-1}{2}$ we may obviously use P$-m$, or simply $-m$; for $(P-m)^2=P^2-2Pm+m^2\equiv m^2$, or, because $(-m)^2=m^2$, b being 2.

(3) Let $10^a\equiv m$, and $10^b\equiv n$; then $10^{a+b}\equiv mn$.

In practice $a+b$ is never greater than P$-$1.

Cor. 1. When m and n are each of them less than P, we may with advantage use $-m$ and $-n$; that is, we may subtract m and n severally from P; for $(P-m)(P-n)=P^2-P(m+n)+mn=(-m)(-n)$.

Cor. 2. When m is $>\frac{P-1}{2}$, and n is $<\frac{P-1}{2}$, or *vice versâ*, we may use

* The part from 17,000 to 20,000 was received January 8, 1874.

$-m$ and n, or *vice versâ*, obtaining a negative result, which becomes positive by being subtracted from P.

(4) Let $2c$ and $3c$, not greater than $\frac{P-1}{2}$, be submultiples of $P-1$; and let $10^c \equiv \pm S$, and $10^{2c} \equiv S-1$; then $10^{3c} \equiv \mp 1$. This is evident from (2) and (3).

From (1) we have $10^{\frac{P-1}{2}} \equiv \pm 1$, according as the submultiple of $P-1$ is even or odd.

On these theorems and adjuncts my calculations have been based. They enable us to find the remainder either from $\frac{10^{P-1}}{P}$, or from any submultiple, such as $\frac{10^{\frac{P-1}{n}}}{P}$, or from any figure in $\frac{10^{P-1}}{P}$, and, if required, the *figure* itself. Compared with other methods, such for instance as Dr. Salmon's *, mine may seem tedious, requiring as it does much multiplication and division. All I can say is, I did not find it so, though I am free to admit that the calculation of such a Table as ours demands very considerable labour.

It would be foreign to my purpose to enter upon the consideration of primitive roots, or even of prime numbers. If we have found 10 to be a primitive root of a great many prime numbers between 10,000 and 20,000, we have contributed something, as far as I know, quite new. In addition to this we have found the number of figures in the period of each of the other primes between 10,000 and 20,000, and have corrected upwards of 70 errors in Burckhardt's and Desmarest's Tables.

I beg to refer to the works of Euler, Lagrange, Legendre, Gauss, Poinsot, Cauchy, and Jacobi (mentioned by Desmarest), and to Desmarest himself, for valuable information touching prime numbers and primitive roots.

I cannot, however, refrain from quoting from Desmarest's 'Théorie des Nombres' the view of Euler as to prime numbers and primitive roots:— "On ne peut saisir entre un nombre premier et les racines primitives qui lui appartiennent, aucune relation d'où l'on puisse déduire *une seule* de ces racines, de sorte que la loi qui règne entre elles paraît aussi profondément cachée que celle qui existe entre les nombres premiers eux-mêmes."

Not discouraged by Euler's remark, Desmarest thus writes:—"Car pourquoi nous serait-il défendu d'ajouter que nous croyons que l'intelligence humaine n'a pas, sur ce point, dit son dernier mot, et que les opérations nombreuses que nous avons dû faire sur les nombres, ne nous ont pas convaincu de l'impossibilité de saisir, sinon l'ensemble, du moins quelques-uns des anneaux de la chaîne mystérieuse qui unit les racines primitives aux nombres premiers."

* *Note by Dr. Salmon.*—The method here referred to is explained, 'Messenger of Mathematics' (1872), p. 49. It is founded on the remark that if we have $10^a \equiv 2^p$, $10^b \equiv 2^q$, we may deduce $10^{aq-bp} \equiv 1$. Thus, let the prime be 251, we can at once write down the equations $10^3 \equiv -2^3$, $2^q \equiv 10^1$, whence immediately $10^{25} \equiv -1$, $10^{50} \equiv 1$. In like manner from the equations $10^a \equiv 2^l 3^p$, $10^b \equiv 2^m 3^q$, $10^c \equiv 2^n 3^r$, we deduce that the number of figures in the period of the reciprocal of the prime is

$$a(mr-nq)+b(np-rl)+c(lq-mp).$$

By the application of these principles I calculated the results obtained in the following Table as far as 18500. For the primes above that number Mr. Shanks is solely responsible; but my experience of his accuracy gives me confidence in his results.

TABLE I. List of Errors in Desmarest's Table.

Primes.	No. of figures in period of reciprocal.		No. of figures in period of reciprocal.	Primes.	No. of figures in period of reciprocal.		No. of figures in period of reciprocal.
3 omitted	omitted	should be	1	5557	1389	should be	926
277	138	"	69	5779	5778	"	2889
317	158	"	79	5827	5826	"	2913
397	198	"	99	6101	3050	"	1220
449	224	"	32	6277	3138	"	1569
787	786	"	393	6287	3143	"	6286
1409	1408	"	32	8421 should be 6421			
1657	276	"	552	6781	6780	"	1356
1733	433	"	866	6997	3498	"	1749
1889	59	"	118	7001	3500	"	1750
2087	$\frac{P-1}{8}$ & $\frac{P-1}{9}$	"	298	7127	509	"	1018
3253	271	"	542	7481	3740	"	748
3373	562	"	843	7561	3780	"	1890
3413	853	"	1706	7717	3858	"	1929
3517	1758	"	879	7741	2580	"	860
3541	60	"	20	7841	392	"	56
3547	3546	"	1773	7853 omitted	omitted	"	3926
3637	3636	"	909	8011	1335	"	2670
3677	919	"	1838	8087	4043	"	8086
3769	942	"	1884	8093	8092	"	4046
3821	1910	"	3820	8101	8100	"	1620
3911	3910	"	1955	8219	4109	"	8218
4049	1012	"	2024	8423	8422, 4211	"	8422
4167 should be 4157				8521	355	"	710
4397	157	"	314	8609	1076 ~~538~~	"	1076
4621	4620	"	924	8681	4340	"	868
4651	2325	"	4650	8893	4446	"	2223
5871 should be 4871				8999	8998	"	4499
4943	2471	"	4942	9067	9066	"	4533
5081	2540	"	1270	9187	9186	"	4593
5107	5106	"	2553	9397	4698	"	81
5407	901	"	1802	9521	952	"	595
5479	5478	"	2739	9629	4814	"	9628
5519	5518	"	2759	9649	1206	"	603
				9941	9940	"	1988

N.B. There are 64 errors, 3 misprints, 2 omissions, viz. 3 and 7853

TABLE II. List of Errors in Burckhardt's and Jacobi's Tables.

	Primes.	No. of figures in period of reciprocal.		No. of figures in period of reciprocal.
Burckhardt and Jacobi ...	911	450	should be	455
Burckhardt	1979	1976	"	1978
Jacobi	3462 should be 3467			
Burckhardt and Jacobi...	1213	1212	"	202
	1597	266	"	133
	1831	915	"	305
	1951	390	"	195
	1993	1992	"	664
	2311	462	"	231
	2437	2436	"	1218
	3467	3466	"	1733

N.B. There are 3 misprints and 8 errors

In the left-hand columns of Table III. are primes; in the right-hand columns, immediately opposite, is the number of figures in the period of the reciprocal of each prime.

TABLE III.

3	1	311	155	691	230	1109	1108	1567	1566
7	6	313	312	701	700	1117	558	1571	1570
11	2	317	79	709	708	1123	561	1579	1578
13	6	331	110	719	359	1129	564	1583	1582
17	16	337	336	727	726	1151	575	1597	133
19	18	347	173	733	61	1153	1152	1601	200
23	22	349	116	739	246	1163	581	1607	1606
29	28	353	32	743	742	1171	1170	1609	201
31	15	359	179	751	125	1181	1180	1613	403
37	3	367	366	757	27	1187	593	1619	1618
41	5	373	186	761	380	1193	1192	1621	1620
43	21	379	378	769	192	1201	200	1627	271
47	46	383	382	773	193	1213	202	1637	409
53	13	389	388	787	393	1217	1216	1657	552
59	58	397	99	797	199	1223	1222	1663	1662
61	60	401	200	809	202	1229	1228	1667	833
67	33	409	204	811	810	1231	41	1669	556
71	35	419	418	821	820	1237	206	1693	423
73	8	421	140	823	822	1249	208	1697	1696
79	13	431	215	827	413	1259	1258	1699	566
83	41	433	432	829	276	1277	638	1709	1708
89	44	439	219	839	419	1279	639	1721	430
97	96	443	221	853	213	1283	641	1723	287
101	4	449	32	857	856	1289	92	1733	866
103	34	457	152	859	26	1291	1290	1741	1740
107	53	461	460	863	862	1297	1296	1747	291
109	108	463	154	877	438	1301	1300	1753	584
113	112	467	233	881	440	1303	1302	1759	879
127	42	479	239	883	441	1307	653	1777	1776
131	130	487	486	887	886	1319	659	1783	1782
137	8	491	490	907	151	1321	55	1787	893
139	46	499	498	911	455	1327	1326	1789	1788
149	148	503	502	919	459	1361	680	1801	900
151	75	509	508	929	464	1367	1366	1811	1810
157	78	521	52	937	936	1373	686	1823	1822
163	81	523	261	941	940	1381	1380	1831	305
167	166	541	540	947	473	1399	699	1847	1846
173	43	547	91	953	952	1409	32	1861	1860
179	178	557	278	967	322	1423	158	1867	933
181	180	563	281	971	970	1427	713	1871	935
191	95	569	284	977	976	1429	1428	1873	1872
193	192	571	570	983	982	1433	1432	1877	938
197	98	577	576	991	495	1439	719	1879	313
199	99	587	293	997	166	1447	1446	1889	118
211	30	593	592	1009	252	1451	290	1901	380
223	222	599	299	1013	253	1453	726	1907	953
227	113	601	300	1019	1018	1459	162	1913	1912
229	228	607	202	1021	1020	1471	735	1931	386
233	232	613	51	1031	103	1481	740	1933	21
239	7	617	88	1033	1032	1483	247	1949	1948
241	30	619	618	1039	519	1487	1486	1951	195
251	50	631	315	1049	524	1489	248	1973	986
257	256	641	32	1051	1050	1493	373	1979	1978
263	262	643	107	1061	212	1499	214	1987	331
269	268	647	646	1063	1062	1511	755	1993	664
271	5	653	326	1069	1068	1523	761	1997	998
277	69	659	658	1087	1086	1531	1530	1999	999
281	28	661	220	1091	1090	1543	1542	2003	1001
283	141	673	224	1093	273	1549	1548	2011	670
293	146	677	338	1097	1096	1553	1552	2017	2016
307	153	683	341	1103	1102	1559	779	2027	1013

TABLE III. (*continued*).

2029	2028	2539	2538	3023	3022	3547	1773	4057	4056
2039	1019	2543	2542	3037	253	3557	254	4073	4072
2053	342	2549	2548	3041	380	3559	1779	4079	2039
2063	2062	2551	425	3049	508	3571	3570	4091	4090
2069	2068	2557	639	3061	204	3581	3580	4093	22
2081	1040	2579	2578	3067	1533	3583	1194	4099	4098
2083	1041	2591	259	3079	1539	3593	3592	4111	2055
2087	298	2593	2592	3083	1541	3607	3606	4127	4126
2089	1044	2609	1304	3089	1544	3613	602	4129	2064
2099	2098	2617	2616	3109	148	3617	3616	4133	1033
2111	1055	2621	2620	3119	1559	3623	3622	4139	4138
2113	2112	2633	2632	3121	156	3631	1815	4153	4152
2129	532	2647	882	3137	3136	3637	909	4157	2078
2131	710	2657	2656	3163	1581	3643	1821	4159	693
2137	2136	2659	886	3167	3166	3659	3658	4177	4176
2141	2140	2663	2662	3169	72	3671	367	4201	75
2143	2142	2670	1335	3181	636	3673	3672	4211	4210
2153	2152	2677	223	3187	177	3677	1838	4217	4216
2161	30	2683	447	3191	29	3691	1230	4219	4218
2179	2178	2687	2686	3203	1601	3697	1232	4229	4228
2203	1101	2689	42	3209	1604	3701	3700	4231	2115
2207	2206	2693	1346	3217	1072	3709	3708	4241	1060
2213	553	2699	2698	3221	3220	3719	1859	4243	2121
2221	2220	2707	1353	3229	1076	3727	3726	4253	1063
2237	1118	2711	1355	3251	3250	3733	933	4259	4258
2239	1119	2713	2712	3253	542	3739	1246	4261	4260
2243	1121	2719	1359	3257	3256	3761	1880	4271	2135
2251	2250	2729	682	3259	3258	3767	3766	4273	1424
2267	1133	2731	2730	3271	1635	3769	1884	4283	2141
2269	2268	2741	2740	3299	3298	3779	3778	4289	2144
2273	2272	2749	916	3301	3300	3793	1264	4297	1432
2281	228	2753	2752	3307	1653	3797	949	4327	4326
2287	762	2767	2766	3313	3312	3803	1901	4337	4336
2293	1146	2777	2776	3319	553	3821	3820	4339	4338
2297	2296	2789	2788	3323	1661	3823	1274	4349	4348
2309	2308	2791	31	3329	832	3833	3832	4357	242
2311	231	2797	699	3331	3330	3847	3846	4363	2181
2333	583	2801	1400	3343	3342	3851	770	4373	1093
2339	2338	2803	1401	3347	1673	3853	963	4391	2195
2341	2340	2819	2818	3359	1679	3863	3862	4397	314
2347	1173	2833	2832	3361	1680	3877	969	4409	551
2351	1175	2837	709	3371	3370	3881	1940	4421	4420
2357	1178	2843	1421	3373	843	3889	1944	4423	4422
2371	2370	2851	2850	3389	3388	3907	1953	4441	2220
2377	264	2857	408	3391	1695	3911	1955	4447	4446
2381	476	2861	2860	3407	3406	3917	1958	4451	4450
2383	2382	2879	1439	3413	1706	3919	653	4457	4456
2389	2388	2887	2886	3433	3432	3923	1961	4463	4462
2393	184	2897	2896	3449	431	3929	491	4481	2240
2399	1199	2903	2902	3457	384	3931	1310	4483	249
2411	2410	2909	2908	3461	3460	3943	3942	4493	1123
2417	2416	2917	1458	3463	3462	3947	1973	4507	751
2423	2422	2927	2926	3467	1733	3967	3966	4513	1504
2437	1218	2939	2938	3469	3468	3989	3988	4517	2058
2441	305	2953	984	3491	698	4001	500	4519	753
2447	2446	2957	1478	3499	318	4003	87	4523	2261
2459	2458	2963	1481	3511	1755	4007	4006	4547	2273
2467	137	2969	371	3517	879	4013	34	4549	1516
2473	2472	2971	2970	3527	3526	4019	4018	4561	2280
2477	619	2999	1499	3529	1764	4021	268	4567	4566
2503	278	3001	1500	3533	1766	4027	2013	4583	4582
2521	630	3011	3010	3539	3538	4049	2024	4591	2295
2531	46	3019	3018	3541	20	4051	4050	4597	2298

TABLE III. (*continued*).

4603	2301	5147	2573	5689	316	6247	6246	6803	3401
4621	924	5153	5152	5693	1423	6257	6256	6823	6822
4637	61	5167	5166	5701	5700	6263	6262	6827	3413
4639	2319	5171	110	5711	571	6269	6268	6829	6828
4643	2321	5179	5178	5717	1429	6271	1045	6833	6832
4649	7	5189	5188	5737	5736	6277	1569	6841	855
4651	4650	5197	433	5741	5740	6287	6286	6857	6856
4657	1552	5209	372	5743	5742	6299	94	6863	6862
4663	222	5227	2613	5749	5748	6301	6300	6869	6868
4673	4672	5231	2615	5779	2889	6317	3158	6871	3435
4679	2339	5233	5232	5783	5782	6323	3161	6883	3441
4691	4690	5237	77	5791	965	6329	3164	6899	6898
4703	4702	5261	1052	5801	1450	6337	6336	6907	1151
4721	2360	5273	5272	5807	5806	6343	6342	6911	3455
4723	2361	5279	2639	5813	2906	6353	6352	6947	3473
4729	1182	5281	2640	5821	5820	6359	3179	6949	6948
4733	1183	5297	5296	5827	2913	6361	1590	6959	3479
4751	2375	5303	5302	5839	2919	6367	6366	6961	3480
4759	2379	5309	5308	5843	2921	6373	1062	6967	6966
4783	4782	5323	2661	5849	1462	6379	2126	6971	6970
4787	2393	5333	1333	5851	1950	6389	6388	6977	6976
4789	228	5347	2673	5857	5856	6397	78	6983	6982
4793	4792	5351	2675	5861	5860	6421	2140	6991	3495
4799	2399	5381	5380	5867	2933	6427	1071	6997	1749
4801	800	5387	2693	5869	5868	6449	1612	7001	1750
4813	802	5393	5392	5879	2939	6451	2150	7013	3506
4817	4816	5399	2699	5881	2940	6469	924	7019	7018
4831	805	5407	1802	5897	5896	6473	6472	7027	1171
4861	972	5413	2706	5903	5902	6481	270	7039	391
4871	2435	5417	5416	5923	2961	6491	1298	7043	503
4877	1219	5419	5418	5927	5926	6521	815	7057	7056
4889	2444	5431	2715	5939	5938	6529	1088	7069	7068
4903	1634	5437	1359	5953	1984	6547	1091	7079	3539
4909	1636	5441	2720	5981	5980	6551	3275	7103	7102
4919	2459	5443	907	5987	2993	6553	6552	7109	7108
4931	4930	5449	2724	6007	858	6563	3281	7121	3560
4933	2466	5471	547	6011	6010	6569	1642	7127	1018
4937	4936	5477	1369	6029	6028	6571	6570	7129	594
4943	4942	5479	2739	6037	3018	6577	2192	7151	275
4951	2475	5483	2741	6043	3021	6581	1316	7159	3579
4957	413	5501	5500	6047	6046	6599	3299	7177	7176
4967	4966	5503	5502	6053	3026	6607	2202	7187	3593
4969	828	5507	2753	6067	3033	6619	6618	7193	7192
4973	226	5519	2759	6073	6072	6637	474	7207	7206
4987	2493	5521	345	6079	1013	6653	3326	7211	1030
4993	1664	5527	5526	6089	761	6659	6658	7213	1803
4999	357	5531	5530	6091	2030	6661	6660	7219	7218
5003	2501	5557	926	6101	1220	6673	6672	7229	7228
5009	626	5563	2781	6113	6112	6679	3339	7237	402
5011	1670	5569	1392	6121	3060	6689	1672	7243	3621
5021	5020	5573	2786	6131	6130	6691	6690	7247	7246
5023	1674	5581	5580	6133	1533	6701	6700	7253	74
5039	2519	5591	2795	6143	6142	6703	6702	7283	3641
5051	50	5623	5622	6151	1025	6709	6708	7297	2432
5059	5058	5639	2819	6163	79	6719	3359	7307	3653
5077	2538	5641	470	6173	3086	6733	3366	7309	7308
5081	1270	5647	1882	6197	3098	6737	6736	7321	3660
5087	5086	5651	5650	9199	3099	6761	1690	7331	1466
5099	5098	5653	2826	6203	443	6763	161	7333	611
5101	1700	5657	5656	6211	6210	6779	6778	7349	7348
5107	2553	5659	5658	6217	6216	6781	1356	7351	1225
5113	1704	5669	5668	6221	6220	6791	679	7369	1842
5119	853	5683	2841	6229	2076	6793	6792	7393	7392

TABLE III. (*continued*).

7411	7410	7937	7936	8543	8542	9103	9102	9649	603
7417	2472	7949	7948	8563	4281	9109	9108	9661	1380
7433	7432	7951	3975	8573	4286	9127	3042	9677	2419
7451	7450	7963	3981	8581	2860	9133	1522	9679	1613
7457	7456	7993	2664	8597	2149	9137	9136	9689	346
7459	7458	8009	2002	8599	1433	9151	1525	9697	9696
7477	3738	8011	2670	8609	1076	9157	4578	9719	4859
7481	748	8017	8016	8623	8622	9161	229	9721	4860
7487	7486	8039	4019	8627	4313	9173	4586	9733	2433
7489	1872	8053	4026	8629	2876	9181	3060	9739	9738
7499	7498	8059	8058	8641	4320	9187	4593	9743	9742
7507	3753	8069	8068	8647	8646	9199	4599	9749	9748
7517	3758	8081	2020	8663	8662	9203	4601	9767	9766
7523	3761	8087	8086	8669	8668	9209	2302	9769	4884
7529	1882	8089	1348	8677	723	9221	9220	9781	9780
7537	2512	8093	4046	8681	868	9227	4613	9787	4893
7541	7540	8101	1620	8689	2172	9239	4619	9791	4895
7547	3773	8111	811	8693	4346	9241	4620	9803	4901
7549	2516	8117	2029	8699	8698	9257	9256	9811	9810
7559	3779	8123	4061	8707	4353	9277	4638	9817	9816
7561	1890	8147	4073	8713	8712	9281	928	9829	9828
7573	631	8161	1020	8719	4359	9283	1547	9833	9832
7577	7576	8167	2722	8731	8730	9293	2323	9839	4919
7583	7582	8171	8170	8737	2912	9311	4655	9851	9850
7589	1084	8179	8178	8741	8740	9319	4659	9857	9856
7591	3795	8191	1365	8747	4373	9323	4661	9859	3286
7603	1267	8209	4104	8753	8752	9337	3112	9871	4935
7607	7606	8219	8218	8761	876	9341	9340	9883	4941
7621	508	8221	2740	8779	22	9343	9342	9887	9886
7639	3819	8231	4115	8783	8782	9349	3116	9901	12
7643	3821	8233	8232	8803	1467	9371	9370	9907	4953
7649	1912	8237	4118	8807	8806	9377	9376	9923	4961
7669	284	8243	4121	8819	8818	9391	4695	9929	1241
7673	7672	8263	8262	8821	8820	9397	81	9931	9930
7681	1920	8269	8268	8831	4415	9403	1567	9941	1988
7687	7686	8273	8272	8837	4418	9413	4706	9949	9948
7691	7690	8287	8286	8839	4419	9419	554	9967	9966
7699	7698	8291	8290	8849	553	9421	9420	9973	554
7703	7702	8293	2073	8861	8860	9431	4715	10007	10006
7717	1929	8297	8296	8863	8862	9433	1048	10009	5004
7723	1287	8311	4155	8867	4433	9437	4718	10037	386
7727	7726	8317	462	8887	8886	9439	1573	10039	5019
7741	860	8329	1041	8893	2223	9461	9460	10061	10060
7753	7752	8353	8352	8923	1487	9463	3154	10067	5033
7757	1939	8363	4181	8929	144	9467	4733	10069	10068
7759	3879	8369	4184	8933	2233	9473	9472	10079	5039
7789	2596	8377	8376	8941	2980	9479	4739	10091	10090
7793	7792	8387	599	8951	4475	9491	9490	10093	2523
7817	7816	8389	8388	8963	4481	9497	9496	10099	3366
7823	7822	8419	2806	8969	4484	9511	1585	10103	10102
7829	7828	8423	8422	8971	8970	9521	595	10111	5055
7841	56	8429	8428	8999	4499	9533	2383	10133	2533
7853	3926	8431	4215	9001	1125	9539	9538	10139	10138
7867	3933	8443	4221	9007	3002	9547	4773	10141	10140
7873	7872	8447	8446	9011	9010	9551	955	10151	5075
7877	3938	8461	2820	9013	2253	9587	4793	10159	5079
7879	3939	8467	4233	9029	9028	9601	4800	10163	5081
7883	3941	8501	8500	9041	1130	9613	267	10169	5084
7901	7900	8513	8512	9043	4521	9619	3206	10177	10176
7907	3953	8521	710	9049	4524	9623	9622	10181	10180
7919	3959	8527	2842	9059	9058	9629	9628	10193	10192
7927	7926	8537	8536	9067	4533	9631	4815	10211	10210
7933	3966	8539	2846	9091	10	9643	4821	10223	10222

TABLE III. (*continued*).

10243	569	10847	10846	11447	11446	12049	6024	12613	6306
10247	10246	10853	5426	11467	5733	12071	355	12619	4206
10253	2563	10859	10858	11471	5735	12073	12072	12637	3159
10259	10258	10861	10860	11483	5741	12097	4032	12641	3160
10267	5133	10867	1811	11489	2872	12101	12100	12647	12646
10271	79	10883	5441	11491	766	12107	6053	12653	6326
10273	10272	10889	2722	11497	11496	12109	4036	12659	12658
10289	5144	10891	1210	11503	11502	12113	12112	12671	181
10301	10300	10903	10902	11519	5759	12119	6059	12689	793
10303	3434	10909	1212	11527	3842	12143	12142	12697	12696
10313	10312	10937	10936	11549	11548	12149	12148	12703	4234
10321	2580	10939	10938	11551	1925	12157	2026	12713	12712
10331	2066	10949	10948	11579	11578	12161	6080	12721	2120
10333	5166	10957	2739	11587	1931	12163	6081	12739	4246
10337	10336	10973	2743	11593	11592	12197	3049	12743	12742
10343	10342	10979	10978	11597	5798	12203	6101	12757	2126
10357	5178	10987	5493	11617	11616	12211	4070	12763	709
10369	2592	10993	10992	11621	11620	12227	6113	12781	12780
10391	5195	11003	5501	11633	11632	12239	6119	12791	6395
10399	1733	11027	5513	11657	11656	12241	6120	12799	2133
10427	5213	11047	11046	11677	5838	12251	12250	12809	6404
10429	948	11057	11056	11681	5840	12253	3063	12821	12820
10433	10432	11059	11058	11689	487	12263	12262	12823	12822
10453	5226	11069	11068	11699	11698	12269	12268	12829	4276
10457	10456	11071	615	11701	11700	12277	3069	12841	6420
10459	10458	11083	1847	11717	2929	12281	6140	12853	459
10463	10462	11087	482	11719	5859	12289	384	12889	3222
10477	1746	11093	2773	11731	11730	12301	2460	12893	3223
10487	10486	11113	3704	11743	11742	12323	6161	12899	12898
10499	10498	11117	2779	11777	11776	12329	3082	12907	6453
10501	3500	11119	5559	11779	3926	12343	4114	12911	6455
10513	10512	11131	11130	11783	11782	12347	6173	12917	6458
10529	5264	11149	11148	11789	11788	12373	6186	12919	2153
10531	10530	11159	5579	11801	2950	12377	12376	12923	6461
10559	5279	11161	310	11807	11806	12379	12378	12941	12940
10567	10566	11171	11170	11813	5906	12391	6195	12953	12952
10589	10588	11173	5586	11821	11820	12401	1240	12959	6479
10597	5298	11177	11176	11827	5913	12409	6204	12967	4322
10601	1060	11197	2799	11831	169	12413	6206	12973	2162
10607	10606	11213	2803	11833	11832	12421	12420	12979	12978
10613	758	11239	5619	11839	5919	12433	4144	12983	12982
10627	5313	11243	5621	11863	11862	12437	6218	13001	1625
10631	5315	11251	2250	11867	5933	12451	12450	13003	6501
10639	5319	11257	11256	11887	11886	12457	12456	13007	13006
10651	10650	11261	2252	11897	11896	12473	12472	13009	2168
10657	10656	11273	11272	11903	11902	12479	6239	13033	4344
10663	10662	11279	5639	11909	11908	12487	12486	13037	6518
10667	5333	11287	11286	11923	5961	12491	12490	13043	6521
10687	10686	11299	11298	11927	11926	12497	12496	13049	3262
10691	10690	11311	377	11933	5966	12503	12502	13063	13062
10709	10708	11317	943	11939	11938	12511	2085	13093	2182
10711	595	11321	1132	11941	11940	12517	149	13099	13098
10723	5361	11329	1888	11953	11952	12527	12526	13103	13102
10729	596	11351	5675	11959	5979	12539	12538	13109	13108
10733	2683	11353	11352	11969	352	12541	4180	13121	6560
10739	10738	11369	812	11971	11970	12547	6273	13127	13126
10753	3584	11383	11382	11981	11980	12553	12552	13147	939
10771	2154	11393	11392	11987	5993	12569	6284	13151	1315
10781	10780	11399	5699	12007	4002	12577	12576	13159	2193
10789	10788	11411	2282	12011	12010	12583	12582	13163	6581
10799	5399	11423	11422	12037	3009	12589	12588	13171	4390
10831	5415	11437	2859	12041	6020	12601	6300	13177	13176
10837	63	11443	1907	12043	2007	12611	12610	13183	4394

TABLE III. (*continued*).

13187	6593	13807	13806	14461	4820	15061	5020	15629	15628
13217	13216	13829	13828	14479	7239	15073	15072	15641	391
13219	4406	13831	2305	14489	7244	15077	7538	15643	7821
13229	13228	13841	6920	14503	14502	15083	7541	15647	15646
13241	1655	13859	13858	14519	7259	15091	3018	15649	489
13249	288	13873	13872	14533	519	15101	604	15661	5220
13259	1894	13877	3469	14537	14536	15107	7553	15667	7833
13267	737	13879	6939	14543	14542	15121	7560	15671	1567
13291	13290	13883	6941	14549	14548	15131	3026	15679	2613
13297	13296	13901	13900	14551	485	11137	15136	15683	7841
13309	4436	13903	13902	14557	2426	15139	15138	15727	15726
13313	13312	13907	409	14561	7280	15149	15148	15731	15730
13327	4442	13913	13912	14563	809	15161	3790	15733	2622
13331	13330	13921	696	14591	7295	15173	7586	15737	15736
13337	13336	13931	13930	14593	14592	15187	7593	15739	15738
13339	13338	13933	6966	14621	14620	15193	15192	15749	15748
13367	13366	13963	6981	14627	7313	15199	7599	15761	394
13381	13380	13967	13966	14629	14628	15217	15216	15767	15766
13397	6698	13997	6998	14633	14632	15227	7613	15773	7886
13399	957	13999	2333	14639	7319	15233	15232	15787	7893
13411	13410	14009	3502	14653	3663	15241	3810	15791	7895
13417	4472	14011	14010	14657	14656	15259	15258	15797	3949
13421	13420	14029	14028	14669	14668	15263	15262	15803	7901
13441	6720	14033	14032	14683	2447	15269	1388	15809	3952
13451	13450	14051	2810	14699	14698	15271	7635	15817	5272
13457	13456	14057	14056	14713	14712	15277	3819	15823	15822
13463	13462	14071	7035	14717	7358	15287	15286	15859	15858
13469	13468	14081	1760	14723	7361	15289	1274	15877	567
13477	6738	14083	7041	14731	14730	15299	15298	15881	7940
13487	13486	14087	14086	14737	14736	15307	7653	15887	15886
13499	13498	14107	2351	14741	14740	15313	5104	15889	3972
13513	4504	14143	14142	14747	7373	15319	7659	15901	15900
13523	6761	14149	524	14753	14752	15329	7664	15907	7953
13537	13536	14153	14152	14759	7379	15331	5110	15913	15912
13553	1936	14159	7079	14767	14766	15349	15348	15919	7959
13567	4522	14173	3543	14771	2110	15359	7679	15923	7961
13577	13576	14177	14176	14779	14778	15361	256	15937	5312
13591	1359	14197	91	14783	14782	15373	1281	15959	7979
13597	3399	14207	14206	14797	3699	15377	15376	15971	15970
13613	6806	14221	2844	14813	7406	15383	15382	15973	121
13619	1238	14243	7121	14821	4940	15391	7695	15991	7995
13627	6813	14249	7124	14827	2471	15401	275	16001	2000
13633	4544	14251	14250	14831	1483	15413	7706	16007	16006
13649	853	14281	1190	14843	7421	15427	7713	16033	16032
13669	13668	14293	1191	14851	990	15439	7719	16057	1784
13679	6839	14303	14302	14867	7433	15443	7721	16061	3212
13681	3420	14321	3580	14869	4956	15451	5150	16063	5354
13687	4562	14323	1023	14879	7439	15461	15460	16067	8033
13691	13690	14327	14326	14887	14886	15467	7733	16069	5356
13693	326	14341	14340	14891	14890	15473	15472	16073	16072
13697	13696	14347	7173	14897	14896	15493	7746	16087	5362
13709	13708	14369	449	14923	7461	15497	1192	16091	16090
13711	6855	14387	7193	14929	1866	15511	7755	16097	16096
13721	6860	14389	4796	14939	14938	15527	15526	16103	16102
13723	6861	14401	3600	14947	7473	15541	740	16111	1611
13729	3432	14407	686	14951	1495	15551	7755	16127	16126
13751	6875	14411	2882	14957	7478	11559	2593	16139	16138
13757	362	14419	4806	14969	7484	15569	7784	16141	3228
13759	6879	14423	14422	14983	4994	15581	15580	16183	16182
13763	6881	14431	555	15013	3753	15583	15582	16187	8093
13781	13780	14437	218	15017	15016	15601	390	16189	852
13789	13788	14447	14446	15031	7515	15607	15606	16193	16192
13799	6899	14449	3612	15053	3763	15619	15618	16217	16216

TABLE III. (*continued*).

16223	16222	16889	8444	17483	8741	18097	6032	18719	9359
16229	16228	16901	3380	17489	2186	18119	9059	18731	3746
16231	8115	16903	5634	17491	17490	18121	2265	18743	18742
16249	1354	16921	423	17497	17496	18127	18126	18749	18748
16253	8126	16927	5642	17509	5836	18131	490	18757	3126
16267	8133	16931	16930	17519	8759	18133	9066	18773	9386
16273	16272	16937	16936	17539	5846	18143	18142	18787	9393
16301	3260	16943	16942	17551	2925	18149	1396	18793	18792
16319	8159	16963	8481	17569	2928	18169	9084	18797	127
16333	4083	16979	16978	17573	4393	18181	6060	18803	9401
16339	16338	16981	16980	17579	17578	18191	9095	18839	9419
16349	16348	16987	8493	17581	17580	18199	9099	18859	18858
16361	1636	16993	16992	17597	4399	18211	18210	18869	18868
16363	8181	17011	17010	17599	2933	18217	18216	18899	18898
16369	264	17021	3404	17609	8804	18223	6074	18911	9455
16381	5460	17027	8513	17623	1958	18229	18228	18913	18912
16411	16410	17029	17028	17627	8813	18233	424	18917	4729
16417	16416	17033	17032	17657	17656	18251	18250	18919	9459
16421	16420	17041	2840	17659	5886	18253	9126	18947	9473
16427	8213	17047	5682	17669	17668	18257	2608	18959	9479
16433	16432	17053	1421	17681	1105	18269	18268	18973	153
16447	16446	17077	2846	17683	8841	18287	18286	18979	18978
16451	16450	17093	4273	17707	8853	18289	9144	19001	2375
16453	2742	17099	17098	17713	17712	18301	18300	19009	4752
16477	4119	17107	8553	17729	1108	18307	3051	19013	4753
16481	2060	17117	4279	17737	17736	18311	9155	19031	9515
16487	16486	17123	8561	17747	8873	18313	18312	19037	9518
16493	4123	17137	5712	17749	17748	18329	4582	19051	6350
16519	2753	17159	8579	17761	8880	18341	18340	19069	6356
16529	8264	17167	17166	17783	17782	18353	18352	19073	19072
16547	8273	17183	17182	17789	17788	18367	6122	19079	9539
16553	16552	17189	17188	17791	8895	18371	3674	19081	795
16561	8280	17191	8595	17807	17806	18379	18378	19087	6362
16567	16566	17203	2867	17827	8913	18397	9198	19121	9560
16573	8286	17207	17206	17837	91	18401	4600	19139	19138
16603	8301	17209	4302	17839	8919	18413	4603	19141	19140
16607	16606	17231	8615	17851	714	18427	3071	19157	4789
16619	16618	17239	8619	17863	17862	18433	6144	19163	9581
16631	8315	17257	17256	17881	2235	18439	9219	19181	19180
16633	5544	17291	17290	17891	17890	18443	9221	19183	6394
16649	8324	17293	1441	17903	17902	18451	6150	19207	19206
16651	3330	17299	1922	17909	17908	18457	18456	19211	3842
16657	16656	17317	1443	17911	2985	18461	18460	19213	9606
16661	16660	17321	8660	17921	8960	18481	1320	19219	19218
16673	16672	17327	17326	17923	8961	18493	4623	19231	9615
16691	16690	17333	8666	17929	8964	18503	18502	19237	4809
16693	2782	17341	5780	17939	17938	18517	3086	19249	3208
16699	16698	17351	8675	17957	8978	18521	9260	19259	19258
16703	16702	17359	2893	17959	8979	18523	9261	19267	9633
16729	697	17377	17376	17971	17970	12539	18538	19273	19272
16741	16740	17383	17382	17977	856	18541	3708	19289	4822
16747	8373	17387	8693	17981	17980	18553	6184	19301	3860
16759	931	17389	17388	17987	8993	18583	18582	19309	19308
16763	29	17393	17392	17989	5996	18587	9293	19319	9659
16787	8393	17401	4350	18013	9006	18593	18592	19333	4833
16811	3362	17417	17416	18041	220	18617	1432	19373	4843
16823	16822	17419	17418	18043	291	18637	9318	19379	19378
16829	16828	17431	8715	18047	2578	18661	18660	19381	1292
16831	8415	17443	8721	18049	1128	18671	1867	19387	3231
16843	401	17449	4362	18059	18058	18679	3113	19391	1385
16871	1205	17467	8733	18061	18060	18691	18690	19403	9701
16879	2813	17471	8735	18077	4519	18701	3740	19417	19416
16883	8441	17477	8738	18089	9044	18713	18712	19421	19420

TABLE III. (*continued*).

19423	3237	19501	780	19681	9840	19777	6952	19913	19912	
19427	9713	19507	9753	19687	19686	19793	19792	19919	9959	
19429	19428	19531	6510	19697	19696	19801	9900	19927	19926	
19433	19432	19541	19540	19699	19698	19813	4953	19937	2848	
19441	1620	19543	19542	19709	19708	19819	19818	19949	19948	
19447	19446	19559	9779	19717	9858	19841	~~1984~~	19961	9980	64
19457	19456	19571	19570	19727	19726	19843	3307	19963	3327	
19463	19462	19577	19576	19739	19738	19853	9926	19973	9986	
19469	19468	19583	19582	19751	9875	19861	6620	19979	19978	
19471	9735	19597	4899	19753	19752	19867	6622	19991	9995	
19477	3246	19603	3267	19759	9879	19889	9944	19993	19992	
19483	9741	19609	9804	19763	9881	19891	6630	19997	9998	
19489	406	19661	19660							

February 26, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The following Papers were read :—

I. "The Winds of Northern India, in relation to the Temperature and Vapour-constituent of the Atmosphere." By HENRY F. BLANFORD, F.G.S., Meteorological Reporter to the Government of Bengal. Communicated by Major-General STRACHEY, R.E. Received May 25, 1873.

(Abstract.)

The object of this paper is to describe the normal wind-currents of Northern India, and their annual variation, and to trace out their origin and causes, so far as these can be discovered in the local physical changes of the atmosphere. After referring to the data on which his conclusions are based, the author goes on to describe the winds of the principal geographical regions of North India in detail.

PART I. *Description of Winds.*

1. *The Punjab.*—As a rule currents from the westward predominate on an average throughout the year; and this is also found to be the case in other parts of North India. In the most northern part of the Punjab, westerly winds prevail in the cold and hot dry months, easterly in the rainy months. In the central districts northerly winds preponderate over southerly, having in the cold months a westerly tendency, but drawing round to the north-east as the hot weather comes on, while as the rainy season sets in the winds tend to east and south-east, returning to west after the rain ceases in September. In the southern part of the Punjab (and this is also the case in Sindh) easterly winds never prevail,

and southerly, south-westerly, and north-westerly winds predominate—the two former in the rainy months, the last in the cold and hot dry season. In the coldest months the wind veers towards the north, and occasionally passes a little to the east of north.

2. *The Gangetic Plain.*—The great chain of the Himalaya, which skirts the northern edge of this region, the elevation of which is between 150 feet above sea-level on the east and 900 feet on the west, determines in a great measure the direction of its prevailing winds; and those from the north-west and south-east much exceed those from other quarters. In the western part of the plain the north-westerly winds somewhat exceed the easterly. In the eastern part the converse holds good. The change from the westerly to the easterly direction accompanies the change from the hot and dry season to the rains, and from easterly to westerly that from the rains to the cold season. In the districts more remote from the mountains the tendency to a westerly direction increases, and occasional south-west winds blow, apparently caused by incursions from the Arabian Sea. During the hot months, and during the day, the westerly winds blow with great force, falling at night; calms are a characteristic of the nights, and prevail most in the colder months. As the hot season advances the easterly winds become more frequent, and attain their maximum in July, when the rain becomes general. The winds veer from the west through the north to east and south-east, the opposite change being more abrupt, and at one place of observation apparently retrograde.

3. *Plateau of Rajpootana.*—This region is somewhat elevated above the Gangetic plain, varying from 800 or 900 feet to 1800 feet above the sea-level. Winds from the west and south-west greatly exceed those from other quarters in the southern districts, commencing as early as February and continuing till November, when they are replaced by northerly and north-easterly winds. The north-east winds are comparatively weak and unsteady, and interrupted by calms. A similarity to the winds of the Southern Punjab may be observed. The northern part of the plateau of Rajpootana partakes more of the character of the Gangetic plain in its winds, the winds of the hot season being chiefly westerly and north-westerly; but the rainy season is accompanied by south-westerly winds, and not by easterly winds as in the Gangetic plain; and easterly winds are always rare.

4. *Central India.*—This region is on an average somewhat less elevated than that last referred to; it is traversed by the high land known as the Satpoora range, and is otherwise considerably broken up into valley and mountain, so that the winds are more influenced by merely local conditions than in the less hilly regions before noticed. Westerly winds on the whole prevail. In the hot months westerly and north-westerly winds predominate. Local inroads of south-westerly winds occur during the rainy months on the north of the Satpoora range, and less strongly on the south of the range; as the rainy season ceases the winds veer

through west to north and north-east, which direction is dominant after November till January, when the northerly tendency fails and southerly winds blow, which again pass into the westerly winds of the hot months. This region participates in the characteristics both of the plains of Northern India and of the Peninsula, which last is under the influence of the true south-west and north-east monsoons. During the cold months and the rainy season respectively, when the two great monsoons are at their height, the winds of the Central India plateau are from the north-east and south-west, while those of the Gangetic plain are from the north-west and south-east, the former blowing to or from the Arabian or Western Sea, the latter to or from the Bay of Bengal, or Eastern Sea. Only in the hot season do the winds approximate, blowing from the dry region to the north-west towards the thermal focus of Central India and Western Bengal.

5. *Western Bengal.*—This region includes the continuation of the plateau of Central India to the margin of the delta of the Ganges, and descends to the Bay of Bengal. The northern part, being a comparatively open tableland, participates greatly as to its winds in the characters of the neighbouring Gangetic plain. The west and north-west winds of the cold months are followed by south-west and south winds, which draw round to south-east during the rainy season, again reverting to north-west through west. Occasional incursions of the south-west monsoon are felt, which are perceptible in the Gangetic valley. On the coast the winds are very different. The west and north-west winds of the interior are quite subordinate. North and north-east winds begin in October, when the south-west monsoon ceases, becoming more northerly with the increasing cold and the strengthening of the land-winds of the interior. Later they again veer towards the east; and the sea-winds blow from south-east in January, and ultimately from the south-west. After September the winds fall back rapidly through south-east and east to north-east. At places removed from the coast the wind is more westerly than on the coast.

6. *Gangetic Delta.*—From its position this region is swept by the currents of air passing between the Gangetic plain and the equatorial ocean. The general course of the winds is as follows:—The winter monsoon becomes well established in November, blowing nearly from the north on the east of the Delta, and from north-west on the west; near the sea the direction is a little east of north. As the season advances the wind draws round towards the west, where it is about February, and eventually backs by south-west to south and south-east, in which direction it blows during the rainy season and till September. In October the winds are chiefly easterly, but unsteady and apt to be stormy, alternating with calms in the earlier part of the month, and passing into north and north-west in the latter part.

7. *Assam.*—The local configuration of this valley no doubt affects its

winds, forming, as it does, an open passage for the monsoons to pass to and from the region north of the Himalaya. The winter monsoon begins in October, when north and north-east winds blow with great steadiness till January, after which westerly winds are felt, chiefly blowing from the south-west, till in June they predominate and continue till September, when they in turn give way to the easterly winds. On the whole the characteristic of Assam is the prevalence of easterly winds, which is here as conspicuous as that of the westerly winds over the Gangetic plain and Punjab.

8. *Arakan Coast.*—The observations in this region are limited to places on the coast. The northerly winds begin in October, with occasional north-west wind, continue till March, or a month later than in the Gangetic delta, after which they work round to the southward, and at length to south-east by south, which is the normal mean direction of the wind along this coast during the south-west monsoon. This mean direction is varied at all times of the year by the land and sea breezes. The changes of the monsoons occur sensibly later on the southern parts of the coast than on the northern; also the southerly winds attain less easting, and the northerly winds less westing, in the south than in the north. In August a sudden drawing of the wind towards the west is observable on this coast (and is also discernible in Bengal and the North-west Provinces of India), followed by a return to the eastward, due apparently to the influence of the true south-west monsoon of the Arabian Sea, then at its height.

Summary.—From the foregoing it will be seen that the winds of Northern India are very different from those of the adjacent seas. Instead of two monsoons from the north-east and south-west alternately prevailing during about equal periods of the year, we find rather three distinct seasons in which special winds prevail, the directions of which mainly depend on the directions and relative positions of the mountain-ranges and plains.

During the cold-weather months, November to January, light westerly and northerly winds blow from the plains of Upper India down the valleys of the Ganges and Indus, and across the tableland of Central India, and join into the north-east monsoon of the Peninsula. The easterly winds of the valley of Assam add to this current.

In April and May, as the hot weather comes on, the winds of Northern India become more westerly and powerful, and take the form of the hot winds, which are not continuous but diurnal, blowing till sun-down and then followed by calms, and prevailing to the eastern limits of the Gangetic delta. At the same time southerly winds are commencing on the coast, and are felt from Sindh across to Bengal, but only at intervals, and feebly except near the sea.

In June the south-west monsoon, being established in the equatorial ocean, sets in round both coasts of the peninsula, penetrates up the

valleys of the Indus, the Nerbudda, and Taptee, carrying a west or south-west current over Central India, and from the Bay of Bengal pouring up the funnel-shaped opening occupied by the Gangetic delta, whence turning westward it passes up the Gangetic valley towards the Punjab, which seems to be the limit of the south-easterly winds, in Afghanistan the dominant winds being westerly even during the summer months. This is the period of the rainy season of Northern India.

In October, as the south-west monsoon ceases, the southerly current is recurved towards the heated region along the Coromandel coast (on which the rainfall is till this season of the year comparatively small), and, blowing as a south-east wind, causes the autumn rains on that coast, which some writers have erroneously attributed to the north-east monsoon. With the gradual cessation of the southerly winds the westerly winds of Northern India again begin, and the cycle of the year is thus completed.

PART II. *Relation of Winds to other Elements of Climate.*

1. *Temperature.*—The seasons of Northern India present three distinct phases:—the *cold season*, from the end of the rains in September to February or March; the *hot season*, characterized by a dry atmosphere and great diurnal range of temperature; and the *rainy season*, in which the temperature is moderately high and equable, and the air very humid. At the close of the rains (the end of September) the temperature of Northern India from the Punjab to the sea is nearly uniform at about 81° or 82°. But evaporation and radiation to a cloudless sky soon reduce the temperature of the interior below that of the maritime regions; and in January the Punjab is about 11° colder than Bengal, the plains of the North-west Provinces being about midway in temperature between the two. In March the advance of temperature in Central India has brought out two thermal foci—one on the west in Rajpootana, and the other on the east in the hilly tracts of Western Bengal. In April the Central-Indian thermal focus is well developed. The mean temperature of Nagpore is 7° above that of Bombay, 13° above the northern Punjab, and 6° above the coast of the Gangetic delta. The hottest region has a mean temperature between 85° and 90°, the Upper-Punjab and Upper-Assam being from 75° to 77°. In May the thermal focus has gone further to the north-west, and lies in the northern part of the Rajpootana plateau. In June it has reached the Punjab, the temperature of which continues to increase, rising to 95° and more; while that of the south of India begins to fall, consequent on the rains, which commence about the middle of the month. In July the Punjab ranges above 90°, while the greater part of Central India is below 85°. After July the temperature again falls, so that by the end of September it is nearly equalized all over Northern India.

To sum up briefly. In the cold weather there are two foci of mini-

mum temperature, the one in the Punjab and the other in Assam, and, with some exceptions, the isothermals nearly conform to the parallels of latitude. In the hot months a focus of heat is formed in Central India, round which the isotherms are bent, the temperature on the coasts and in the northern plains being considerably lower than that of the interior. Finally, during the rainy season the seat of highest temperature is in the Punjab, the coolest regions then being those of the maximum rainfall, and consisting of two tracts extending from the coasts of Bombay and Bengal, along the course of the monsoon currents.

The author then refers to the distribution of temperature in a vertical direction, as ascertained from observations made at the mountain-stations. He points out apparent anomalies in the differences of temperature due to difference of altitude in the mountains of North-western India and those bordering on Bengal, and suggests, as a probable explanation, the variation of hygrometrical condition of the air in the two regions, remarking that the continual upward diffusion and condensation of water vapour must tend to equalize the upper and lower temperatures, and that this tendency will be the greater as the approach to saturation is closer. The subject, however, is admitted to be one that requires further examination, and particularly with respect to the operation of nocturnal radiation and diurnal absorption of heat—the remark being also made that the available observations give the local temperature near the surface of the mountains, and do not properly represent the condition of the free atmosphere at corresponding elevations.

2. *Vapour-tension, Humidity, and Rainfall.*—In the regions under discussion, the lowest vapour-tension occurs almost everywhere in January, when the temperature is lowest. The lowest mean tension for any month is about 0·2 inch, observed in the Southern Punjab, the corresponding minimum in Bengal being about 0·5 inch. The increase of tension begins early in the districts near the sea, and continues regularly and rapidly till the setting in of the rains; but in the drier regions of the interior, where the west winds prevail throughout the spring and hot-weather months, the rise of tension is slow, probably not more than is due to the actual rise of temperature acting on the local vapour supply. The increase at the commencement of the rains, in June or July, when the southerly winds begin to be felt, is very marked and sudden; and equally so is the fall after September, when the southerly is replaced by the northerly current.

As regards variation of tension due to elevation, the conclusions of former observers are confirmed, that the ratio of decrement follows generally the increase of elevation, but with a marked addition to the relative tensions at the higher stations in the hottest and wettest months.

Passing to the humidity of the air, it is shown that the period of greatest dryness falls *later* in the year the *greater* the distance from the sea, measuring along the course of rain-carrying wind-current. On the coast

of Bengal the driest month is January, the period being later as we go inland up the Gangetic valley, till it is found in May or June in the Punjab and North-west Provinces.

In the western part of the Gangetic valley and the Punjab there is a secondary minimum of dryness, which follows a converse rule to that of the principal minimum; that is, it falls *earlier* the *greater* the distance from the sea in the sense before explained. In the Punjab this minimum is as early as September or October, shortly after the cessation of the rains, gradually advancing till November at Benares, east of which it is not appreciable. Intermediate between the two minimum periods is a secondary or winter maximum, evidently related to the winter rains of the Upper Provinces, and, like the corresponding winter maximum and rains of Europe, traceable to the descent of the equatorial (here the anti-monsoon) current, and the low winter temperature.

The relative humidity of the air remains pretty constant at all elevations on the Himalaya, as already pointed out by Dr. Hooker and other writers, but not including Tibet, the conditions of which are very different. There are, however, considerable exceptions to the general rule; and the local law of variation depends much on local conditions.

The rainfall is next discussed. The author points out that there are three principal seasons of rain calling for notice. The summer and early autumn rains (that is, those of the south-west monsoon, or of *the rainy season* commonly so called) are the most important. In Bengal they begin on an average about the middle of June, with a fall of from 9 to 15 inches in that month. In the Upper Provinces they are later, and in Rajpootana there is little rain till July. Everywhere they have their maximum in July. In the Upper Provinces and Rajpootana very little rain falls in October, and the rains may be said to end in the last week of September. In Bengal and Central India the fall is still considerable in October; and the rains there end about the middle of this last-named month.

The spring rains prevail in the region over which the sea winds blow from the Bay of Bengal early in the year. In Assam and Eastern Bengal showers are frequent in March, and in April the fall is copious, amounting, in those districts to windward of the eastern mountains, to 12 or 14 inches in the latter month. In Western Bengal the fall is less, and takes place with occasional thunder-storms, locally known as *north-westers*, which extend as far inland as Nagpoor and Benares.

The winter rains are received most regularly and copiously in the Punjab and Upper Provinces, Assam, and Cachar. In Bengal and the lower part of the Gangetic valley they are less regular and lighter. They begin at the end of December, continuing till March in the North-west Provinces, and till April in the Punjab. The fall in these districts amounts to about 4 or 5 inches during the whole season. The author considers that, as they do not coincide either with the period of greatest cold or

greatest humidity, these rains must be due to some other cause, which he thinks to be the humidity of the anti-monsoon current.

On the mountains the heaviest rainfall is on the lower and outer slopes. The greatest recorded falls are those at Cherra-Poonji over Eastern Bengal, averaging more than 500 inches in the year. On the Himalaya the records show falls of from 280 inches on the east to 70 or 80 inches in the North-west Provinces, and 40 to 50 inches in the Punjab. Local circumstances of position greatly affect the quantity.

Generally the quantity of rainfall diminishes with increase of distance from the coast; but it increases on approaching a hill-range on the windward side when the rise is steep, while to leeward a decrease takes place, followed eventually by another gradual increase.

3. *Atmospheric Pressure.*—The available data for discussing this part of the subject are imperfect; and particularly the means of reducing the pressures to the sea-level are not forthcoming in many cases. The following remarks are made subject to this explanation.

The mean pressure, reduced to sea-level, in the month of October is nearly uniform over Bengal, on both sides of the bay, in the Central Provinces, and the Gangetic valley, with a slight tendency to a higher pressure in the North-west Provinces and Cuttack on the one side, and on the Arakan coast on the other, which finds its expression in the slightly converging winds of that season.

In the following months the pressure rises over the whole area, but most in the North-west Provinces and Western Bengal; and in December an axis of maximum pressure lies on a line drawn from Cuttack to the North-west Provinces in a north-west and south-east direction. The distribution of pressure remains much the same till the end of February. In March a rapid fall takes place in Northern India; but the line of higher pressure still remains, extending now from North-western India across to the coast of Arakan round the delta of the Ganges. This, doubtless, is the immediate cause of the back to back winds described in Part I.

In April, with a continued rapid fall, a trough of low pressure becomes apparent, which extends from the head of the delta of the Ganges into Central India. In May this area of low pressure is somewhat displaced towards the north, occupying a line from Western Bengal to Nagpoor, along the 24th parallel of latitude. In June the conditions are generally similar, but with much reduced pressure in the Punjab, in the north-west of which province the absolute minimum is probably to be found. The mean difference of pressure in June between Port Blair in the Bay of Bengal and the upper part of the North-west Provinces is not less than $\frac{3}{10}$ of an inch, and between Port Blair and Calcutta $\frac{2}{10}$ of an inch, Calcutta being about as far from Port Blair as from the head of the Gangetic valley, 800 miles; the baric gradient over the Bay of Bengal, therefore, is about double what it is over the axis of the Ganges valley, and amounts

to $\frac{1}{10}$ of an inch for 400 miles over the land, and $\frac{2}{10}$ of an inch over the sea, which suffices to maintain the steady current of the south-west monsoon.

In July the minimum of pressure is reached without important relative change. In August a rise begins, greater over Northern India, which continues during September and October, when the uniformity of pressure is once more approximately restored.

It is apparent that the distribution of pressure follows, within certain limits, that of temperature, in an inverse ratio of intensity. Thus the region of high pressure in the cold months, which lies across Northern India from Roorkee to Cuttack, coincides approximately with the area over which the isothermals, then approximately parallel to the circles of latitude, are bent downwards, or the temperature of which is lowest relatively to the areas to the east and west of it. Again, during the hot half of the year, as the isothermal lines advance, first turning their branches southwards and leaving an area of higher temperature in Central India between them, which eventually is inverted towards the west over the Punjab, so a somewhat corresponding change occurs in the lines of equal pressure, which at this season may be said to be distributed on lines generally following the meridians, but with a loop more or less deeply concave towards the west.

The author then discusses at some length the manner in which these changes of pressure arise, and to what extent they are dependent on the changes in the proportion of aqueous vapour in the air, and concludes that the vapour indirectly greatly influences the pressure by carrying heat from the lower to the upper strata, and by arresting solar and terrestrial radiation, thus equalizing the temperature of the air-column, but that its power of changing the density, by reason of the displacement of the heavier air-particles, is relatively small, and in some cases unimportant. In general terms the changes of temperature are the principal causes of the variations of pressure.

4. *Certain Effects of Winds.*—Inquiry is made whether any *dynamic* heating or cooling of the air can be traced by reason of winds descending to a lower or rising to a higher level, with the conclusion that no such effects are discernible, and that certain explanations given of the winds of India (by other writers), based on such a conception, are erroneous.

Evidence is adduced which is held to establish that *anti-monsoon* currents blow in the upper strata of the atmosphere, at the various seasons of the year, and at varying elevations, causing corresponding modifications in the general temperature, the operation of all winds being to distribute the temperature peculiar to them. To the descent of the *anti-monsoon* current from the south, the author is disposed to attribute the rains of the cold weather.

Attention is also directed to the greater velocity of the wind-currents near the sea, the westerly winds increasing in force as they approach

Bengal, and the south-easterly winds diminishing in force as they reach the North-west Provinces, indicating that descending and ascending currents must be formed in the upper strata, though of the return southward of any descending current from the north there is no direct evidence.

II. "Note on Displacement of the Solar Spectrum." By J. H. N. HENNESSEY, F.R.A.S. Communicated by Professor G. G. STOKES, Sec. R.S. Received December 15, 1873.

The following experiments were made with the (new) spectroscope (three prisms) of the Royal Society to ascertain for this instrument the amount of displacement in the solar spectrum from change of temperature. The spectroscope was set up on a pillar within a small tent at a time of the year when the thermal range is considerable: the collimator was placed horizontal, and directed through a window in the tent to a heliostat, which was made to reflect the sun's image when required. On closing the window darkness prevailed in the tent, so that the bright sodium lines were easily obtained from a spirit-lamp. Before commencing, the slit was adjusted and the spectroscope clamped; and no movement of any kind was permitted in the instrument during the experiments. The displacement was measured by means of a micrometer in the eye-end of the telescope, reading being taken (out of curiosity) successively to both dark and bright lines, *i. e.* to K $1002{\cdot}8 = D_r$ and K $1006{\cdot}8 = D_v$. A verified thermometer was suspended directly over and almost touching the prisms. The meteorological observatory referred to was some fifty yards north of the tent.

Rejecting observation 5 (in the following Table) because the thermometer was evidently in advance of the prisms, we deduce

By Dark lines, displacement equal
D_r to D_v is produced by.... 31°·3 change of temperature.
By Bright lines, displacement equal
D_r to D_v is produced by.... 29·4 „

Mean.... 30

from which it appears that the displacement in question may not be neglected in investigations made under a considerable thermal range.

At Dehra Doon, Lat. N. 30° 20′, Long. E. 78° 9′, Height 2200 feet.

No. of observation.	1872, April 9.	In tent. Vernier Reading.	In tent. Micrometer-readings in divisions. To dark lines of solar spectrum. D_v.	D_r.	To bright lines from spirit-lamp. D_v.	D_r.	Temperature near prisms.	In Meteorological Observatory. Barometer.	Thermometers. Attached.	Air.	In sun's rays.
	h m							inches.			
1.	6 30 A.M.	8760 constant.	88·1	100·1	87·4	98·7	60°·5	27·659	61°·2	60·7	Shade
2.	9 26 „		82·4	93·2	81·3	91·8	81·7	·725	69·9	75·4	85°·7
3.	11 55 „		76·0	87·4	74·0	85·9	92·5	·737	79·8	82·6	100·0
4.	1 25 P.M.		73·8	85·8	71·1	83·5	95·4	·717	82·6	85·5	95·0
5.	4 10 „		72·9	85·2	71·0	84·0	90·7	·684	84·2	84·3	Shade.

From the above we find, mean value of space D_r to D_v by dark lines=11·7 divisions; by bright lines=11·8 divisions. Taking mean micrometer-readings for each observation, we get

	Dark lines, $\frac{D_r+D_v}{2}$.	Bright lines, $\frac{D_r+D_v}{2}$.	Temp.
1.	94·1	93·1	60°·5
2.	87·8	86·6	81·7
3.	81·7	80·0	92·5
4.	79·8	77·3	95·4
5.	79·1	77·5	90·7

III. "On White Lines in the Solar Spectrum." By J. H. N. HENNESSEY, F.R.A.S. Communicated by Professor STOKES, Sec. R.S. Received December 8, 1873.

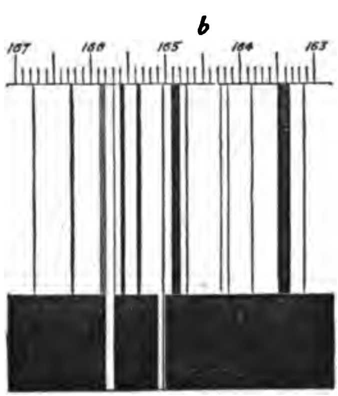
Extract from a Letter from Mr. Hennessey to Professor Stokes.

"Mussoorie, Nov. 12, 1873.

"MY DEAR SIR,—As I cannot account for what is described and drawn in enclosed, I hasten to place the same before you, intending to look for the white lines in question so soon as I move down to a lower altitude. Amongst others, no doubt Kirchhoff closely examined the region in question, without notice of the lines; and this only adds to my perplexity, unless what I see here is due (1) to altitude, or (2) is instrumental. In the latter case I cannot account for the absence of the white lines at Dehra, where I examined the spectrum generally several times; I must, however, add that without close examination and some experience, the lines might easily be passed over. But if instrumental, to what are they due? I very much regret that the old spectroscope is not available at present [it had been temporarily sent elsewhere for a special object] to enable me to verify the phenomena. . . . . ."

[In the drawing sent by Mr. Hennessey, the intervals between the dark lines are coloured green, except in the place of the two white lines. To transfer this distinction to a woodcut, an additional horizontal band has been added below, in which only those parts of the drawing which are left white appear as white, while in the upper part the white of the woodcut represents the white or green, as the case may be, of the original.—G. G. S.]

Part of Solar Spectrum, drawn to Kirchhoff's scale, observed at Mussoorie, N. W. Provinces, India, Lat. N. 30° 28', Long. E. 78° 4'; Height 6700 feet above sea (about), with the Spectroscope belonging to the Royal Society.



Note for diagram.—In course of studying the solar spectrum for atmospheric lines, with an excellent 3-prism (new) spectroscope belonging to the Royal Society, I gradually extended my search, begun at the red end, until on arrival at the region about *b* my attention was attracted by the fact that K 1657·1 by no means appeared as the strong line depicted in Kirchhoff's map, Plate II. On examining this region carefully, I was surprised to find the colourless lines shown in the diagram; these lines, from want of a more appropriate name, I shall call white lines (or spacès); they cannot absolutely be described as bright lines, yet they closely resemble threads of white floss silk held in the light. The spectroscope in use, with the most convenient highest-power eyepiece, presents images of about two thirds to seven ninths of those drawn in the diagram; the former are exaggerated by reckoning to agree with Kirchhoff's millimetre scale; it will therefore be readily understood that the white lines do not present striking objects in the spectroscope, especially about the time of sunset, when I happened first to notice them; they are best seen about noon, when their resemblance to threads of white floss silk is very close; but once seen, the lines in question can always be readily detected. So far as my instrumental means permit, the wider line extends between K 1657·1 and K 1658·3; more accurately speaking, it falls short of the latter and rather underlies the former; the narrower white line is underneath K 1650·3, sensibly more of the former appearing beyond the edge towards violet of the latter, which presents the quaint look of a black line on a white surface enclosed in a green band. These are the only white lines in the spectrum from extreme red to F; they are not bright (or reversed lines), so far as I have had opportunity to judge. Were they bright lines, the question would arise, why these alone should be reversed at 6700 feet above sea. Like the black lines the white lines grow dim and disappear with the slit opened wide. As seen here, K 1657·1 is sensibly weaker than K 1667·4, whereas Kirchhoff assigns 5 *b* to the former and only 3 *a* to the latter.

A Photograph of the Moon, sent by the Rev. Dr. Robinson, F.R.S., taken with the Great Melbourne Equatorial, was exhibited; also a lithograph of the Nebula in Argo, made from eye-observations with the same instrument.

Presents received, February 5, 1874.

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February 19, 1874.

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February 26, 1874.

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February 26, 1874.

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Amsterdam :—Koninklijke Akademie van Wetenschappen. Verhandelingen. Deel XIII. 4to. 1873. Verslagen en Mededeelingen. Afdeeling Natuurkunde. Tweede Reeks. Deel VII. 8vo. 1873. Afd. Letterkunde. Tweede Reeks. Deel III. 8vo. 1873. Jaarboek voor 1872. 8vo. Processen-Verbaal, 1872–73. 8vo. Gaudia Domestica. 8vo. 1873. The Academy.

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Cunningham (D. D.) Microscopic Examinations of Air. folio. *Calcutta* 1873. The Government of India.

Day (F.) Report on the Freshwater Fish and Fisheries of India and Burma. 8vo. *Calcutta* 1873. The Government of India.

Jevons (W. Stanley), F.R.S. The Principles of Science: a Treatise on Logic and Scientific Method. 2 vols. 8vo. *London* 1874. The Author.

Rütimeyer (L.) Ueber den Bau von Schale und Schädel bei lebenden und fossilen Schildkröten. 8vo. *Basel* 1873. The Author.

March 5, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read as follows:—

Rev. Alfred Barry, D.D., D.C.L.
Edward Middleton Barry, R.A.
Isaac Lowthian Bell, F.C.S.
George Bishop, F.R.A.S.
W. T. Blanford, F.G.S.
Henry Bowman Brady, F.L.S.
Thomas Lauder Brunton, M.D.
George Buchanan, M.A., M.D.
Walter Lawry Buller, Sc.D.
W. Chimmo, Capt. R.N.
Prof. W. Kingdom Clifford.
Cuthbert Collingwood, M.A., F.L.S.
Herbert Davies, M.D.
August Dupré, Ph.D., F.C.S.
Thomas Fairbairn.
Joseph Fayrer, M.D.
Prof. David Ferrier, M.D.
Peter Le Neve Foster, M.A.
Augustus Wollaston Franks, M.A.
Prof. Thomas Minchin Goodeve, M.A.
Lewis Dunbar Brodie Gordon, F.G.S.
Robert Baldwin Hayward, M.A.
Prof. Olaus Henrici, Ph.D.
Prescott G. Hewett, F.R.C.S.E.
John Eliot Howard, F.L.S.
Prof. Thomas M'Kenny Hughes, M.A.
Edmund C. Johnson.
Robert M'Lachlan, F.L.S.
Sir Henry Sumner Maine, C.S.I., LL.D.
Richard Henry Major.
William Mayes, Staff-Commander R.N.
Charles Meldrum, M.A.
Edmund James Mills, D.Sc.
Richard Norris, M.D.
Oliver Pemberton, M.R.C.S.
Rev. Stephen Joseph Perry.
John Arthur Phillips, F.C.S.
William Overend Priestley, M.D.
William Chandler Roberts, F.C.S.
Henry Wyldbore Rumsey, M.D.
Henry Young Darracott Scott, Major-General R.E., C.B.
Alfred R. C. Selwyn (Geol. Survey, Canada).
Samuel Sharp, F.G.S.
Robert Swinhoe.
Sir Henry Thompson, F.R.C.S.
Thomas Edward Thorpe, Ph.D.
Charles Todd (Obs., Adelaide).
Edwin T. Truman, M.R.C.S.
Francis Henry Wenham, F.R.M.S.
Wildman Orange Whitehouse, C.E.
Charles William Wilson, Major R.E.
Archibald Henry Plantagenet Stuart Wortley, Lieut.-Col.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

"The Localization of Function in the Brain." By DAVID FERRIER, M.A., M.D., M.R.C.P., Professor of Forensic Medicine, King's College, London. Communicated by J. BURDON SANDERSON, M.D., F.R.S., Professor of Practical Physiology in University College. Received February 20, 1874.

(Abstract.)

The chief contents of this paper are the results of an experimental investigation tending to prove that there is a localization of function in special regions of the cerebral hemispheres.

In a former paper published by the author in the 'West Riding Lunatic Asylum Medical Reports,' vol. iii. 1873, the results were given of experiments on rabbits, cats, and dogs, made specially for the purpose of testing the theory of Hughlings Jackson, that localized and unilateral epilepsies are caused by irritation or "discharging lesions" of the grey matter of the hemispheres in the region of the corpus striatum. Besides confirming Hughlings Jackson's views, the author's researches indicated an exact localization in the hemispheres of centres, or regions, for the carrying out of simple and complex muscular movements of a definite character, and described by him as of a purposive, or expressional, nature.

Facts were also recorded tending to show that other regions of the brain were connected with sensory perception, but no localization was definitely arrived at.

Among the experiments now related are some in further confirmation and extension of those already made on cats, dogs, and rabbits, as well as a new series of experiments on other vertebrates. In particular, numerous experiments on monkeys are described, for the purpose of which the author received a grant of money from the Council of the Royal Society. In addition, the results of experiments on jackals, guineapigs, rats, pigeons, frogs, toads, and fishes are narrated.

The method of investigation consists in the application of the stimulus of an induced current of electricity directly to the surface of the brain in animals rendered only partially insensible during the process of exploration, complete anæsthesia annihilating all reaction. It is supplemented by the method of localized destructive lesions of the hemispheres.

Special attention is called to the precision with which a given result follows stimulation of a definite area—so much so, that when once the brain has been accurately mapped out, the experimenter can predict with certainty the result of stimulation of a given region or centre. The theory that the phenomena are due not to excitation of cortical centres, but to conduction of the electric currents to basal ganglia and motor

tracts, is considered to be disposed of by the fact of the precision and predictable characters of the results, and by the marked differences in the phenomena which are observed when regions in close local relation to each other are excited. Other facts are pointed out bearing in the same direction; among others, the harmony and homology subsisting between the results of experiment in all the different animals.

The experiments on monkeys are first described.

Reference is continually made in the description to figures of the brain, on which are delineated the position and extent of the regions, stimulation of which is followed by constant and definite results. A complete statement of these results in the present abstract is impossible.

Generally, it may be stated that the centres for the movements of the limbs are situated in the convolutions bounding the fissure of Rolando, viz. the ascending parietal convolution with its postero-parietal termination as far back as the parieto-occipital fissure, the ascending frontal, and posterior termination of the superior frontal convolution. Centres for individual movements of the limbs, hands, and feet are differentiated in these convolutions.

Further, in the ascending frontal convolution, on a level with the posterior termination of the middle frontal, are centres for certain facial muscles, *e. g.* the zygomatics &c. At the posterior termination of the inferior frontal convolution and corresponding part of the ascending frontal are the centres for various movements of the mouth and tongue. This is the homologue of "Broca's convolution." At the inferior angle of the intraparietal sulcus is the centre for the platysma.

In the superior frontal convolution, in advance of the centre for certain forward movements of the arm, as well as in the corresponding part of the middle frontal convolution, are areas, stimulation of which causes lateral (crossed) movements of the head and eyes and dilatation of the pupils.

The antero-frontal region, with the inferior frontal and orbital convolutions, give no definite results on irritation. Extirpation of these parts causes a condition resembling dementia.

No results could be ascertained as regards the function of the central lobe or island of Reil.

Irritation of the angular gyrus (*pli courbe*) causes certain movements of the eyeballs and pupils. Destruction of this convolution gives data for regarding it as the cerebral expansion of the optic nerve, and, as such, the seat of visual perception.

The phenomena resulting from irritation of the superior temporo-sphenoidal convolution (pricking of the ear, &c.) are indications of excitation of ideas of sound. It is regarded as the cerebral termination of the auditory nerve. The sense of smell is localized in the uncinate convolution. The situation of the regions connected with sensations of taste and touch is not accurately defined, but some facts are given indicating their probable locality.

The occipital lobes do not react on stimulation. Destruction of these lobes caused no loss of sensation or voluntary motion, but an apparent abolition of the instincts of self-preservation.

The corpora striata are shown to be motor in function, and the optic thalami sensory.

Stimulation of the corpora quadrigemina causes dilatation of the pupils, opisthotonic contractions, and the utterance of peculiar cries when the *testes* alone are irritated. The nature and signification of these phenomena are regarded as still obscure, and requiring further investigation.

Some experiments have been made on the cerebellum of monkeys. They confirm the author's previous views as to the relation of this organ to coordination of the optic axes, and the maintenance of bodily equilibrium. The experiments are not detailed, as they will form the subject of a future paper.

New experiments on dogs essentially confirm those already published, while many new facts have been elicited. Those on jackals agree in the main with the experiments on dogs, both as to the character of the results and the localization of the centres. New experiments on cats generally confirm, as well as further define, the results described by the author in his former paper. The facts of experiment on rabbits, guineapigs, and rats are essentially alike, and also confirm former statements.

In all those animals the sensory regions are defined and their position compared with those in the brain of the monkey.

The only result obtained by stimulation of the cerebral hemispheres in pigeons was contraction of the pupil. The region associated with this action, situated in the postero-parietal aspect, is compared with a similar region in the mammalian brain, and regarded as the seat of visual perception.

Movements of the limbs in frogs, and of the tail and fins in fishes (as in swimming), can be excited from the cerebral hemispheres in these animals. Exact localization of motor and sensory centres is not possible.

The optic lobes in birds, frogs, and fishes seem related to movements of flight and progression, in addition to their relation with the eyes. Similar phenomena result from irritation of the cerebellum; but the signification of these is reserved for future inquiry.

From the data of physiological experiment a foundation is obtained for constructing an anatomical homology of the convolutions.

Among other points in homology the fissure of Rolando is shown to be the homologue of the crucial sulcus in the brain of the Carnivora.

The whole brain is regarded as divided into sensory and motor regions, corresponding to the anatomical relation of these regions to the optic thalami and corpora striata and the sensory and motor tracts.

The motor regions are regarded as essential for the execution of voluntary movements, and as the seat of a corresponding motor memory

motor ideas), the sensory regions being looked upon as the organic seat of ideas derived from sensory impressions. An explanation is attempted of the phenomena of aphasia, and the relation of the memory of words to the ideas they represent.

The theory that a certain action, excited by stimulation of a certain centre, is the result of a mental conception is considered and disputed. From the complexity of mental phenomena, and the participation in them of both motor and sensory substrata, any system of localization of mental faculties which does not take both factors into account must be radically false. A scientific phrenology is regarded as possible.

The paper concludes with a short consideration of the relation of the basal ganglia to the hemispheres. The view is adopted that they constitute a subvoluntary or automatic sensori-motor mechanism.

March 12, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Contributions to the Developmental History of the Mollusca. Sections I., II., III., IV." By E. Ray Lankester, M.A., Fellow of Exeter College, Oxford. Communicated by G. Rolleston, M.D., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford. Received January 19, 1874.

(Abstract.)

Section I. *The ovarian Egg and early development of* Loligo.

The points of greatest interest to which the author draws attention in the present memoir are :—

1. The explanation of the basketwork structure of the surface of the ovarian egg by the plication of the inner egg-capsule.

2. The increase of the yelk by the inception of cells proliferated from the inner egg-capsule.

3. The homogeneous condition of the egg at fertilization.

4. The limitation of yelk-cleavage to the cleavage-patch.

5. The occurrence of independently formed corpuscles (the autoplasts) which take part in the formation of the blastoderm.

6. The primitive eye-chamber, formed by the rising up of an oval wall and its growing together so as to form a roof to the chamber.

7. The origin of the otocysts by invagination.

8. The rhythmic contractility of a part of the wall of the yelk-sac.

9. The disappearance of the primitive mouth, and the development of a secondary mouth.

10. The development of a pair of large nerve-ganglia by invagination of the epiblast immediately below the primitive eye-chambers.

General Considerations relative to the Observations contained in Sections II., III., IV. (*containing the developmental histories of* Pisidium, Aplysia, Tergipes, Polycera, *and* Neritina).

In these observations the author points out briefly their bearing on two matters of theoretical importance, viz. (1) the origin and significance of what has been called the *Gastrula*-phase of development, and (2) the homologies or homogenies (as the author prefers to say) of the shells, ligaments, and internal pens of the Mollusca. More facts have to be sought out and brought to bear on these questions; but the author, while occupied in that further search, indicates the anticipations which must guide and stimulate it. Before doing so he mentions that there are a variety of other matters of interest in the facts recorded in the paper which cannot yet be brought into any theoretical structure, but which are not on that account kept back, as they will probably be of some service in their isolated condition.

Kowalevsky was the first to describe, in a precise manner, the formation of the foundations of the alimentary tract in a developing embryo, by invagination of the wall of a simple primitive blastosphere, or hollow ball of embryonic cleavage-corpuscles. He detected this mode of development in *Amphioxus*, and subsequently in *Ascidia*. By later researches he was able to indicate the same mode of development in certain Vermes (*Sagitta*, *Euaxes*, *Lumbricus*); and he mentioned incidentally that he had observed a similar development in the Heteropodous mollusk *Atalanta*. At that time the author was studying the development of *Pisidium* and *Limax*, and obtained evidence of the invagination of the primitive blastosphere in those two widely separated mollusks. Subsequently at Naples he found the same process occurring in Nudibranchs. The probable identity of this process of invagination with that so well known in the Batrachians, especially through Stricker's admirable work on the subject, became clear, to those occupied with embryological studies, from the facts established by Kowalevsky; and the "anus of Rusconi" could now be recognized in the "orifice of invagination" present in members of the three large groups of Vermes, Mollusca, and Vertebrata.

The embryonic form produced by this invagination-process is a simple sac composed of an ectoderm and endoderm, with an orifice connecting the exterior with the cavity lined by the endoderm. It, in short, presents the typical structure of the simplest Cœlenterata, and corresponds exactly with the so-called *Planula* of the polyps and corals. Hence we are tempted to see in this primitive invagination-form the representative of the Cœlenterate phase of development of the whole animal kingdom.

In a paper published in May 1873*, containing the substance of lectures delivered in the preceding October, the author discussed this notion at some length, and other points connected with the attempt to work out the correspondences of the embryonal cell-layers of the various groups of the animal kingdom. At the end of the year 1872, Professor Häckel's splendid Monograph of the Calcareous Sponges appeared, in which the same questions are methodically discussed. The name *Gastrula* is given by Professor Häckel to the embryonic form which the author proposed to designate by the old name *Planula*; and the multicellular blastosphere, from which the *Gastrula* is developed, which the author had proposed to speak of as a *Polyplast*, he well christens the *Morula*. Professor Häckel was able to show in his monograph that the Calcareous Sponges exhibit a beautifully definite *Gastrula*-larva, which swims freely by means of cilia. Lieberkühn, Miklucho-Maclay, and Oscar Schmidt had previously shown that certain sponges exhibit such an embryonic form; but Professor Häckel described it in many cases, and showed fully its mode of development and structure.

This brings us to an important point in what Häckel calls the "Gastræa theory"†. The *Gastrula* form of the Calcareous Sponges is *not* formed by invagination, but without any opening in the blastosphere making its appearance; the cells constituting its walls divide into an endoderm and an ectoderm; then, and not until then, an orifice is formed from the central cavity to the exterior by a breaking through at one pole. Careful accounts of the development of Cœlenterata, with a view to determine the mode of development of the *Planula* or *Gastrula* form in regard to the question of invagination, are not to hand in a large number of cases. But, on the one hand, we have Kowalevsky's account of the development of *Pelagia* and *Actinia*, in which the formation of a *Gastrula* by invagination is described, as in the cases already cited among Vermes, Mollusca, and Vertebrata; on the other hand, we have Allman's observations on the Hydroids, Schultze's on *Cordylophora*, Kleinenberg's on *Hydra*, Häckel's on the Siphonophora, and Hermann Foll's on the Geryonidæ, in which the ectoderm and endoderm of the embryo (which is at first a *Planula* without mouth, then a *Gastrula* with a mouth) are stated to arise from the splitting or "delamination" of a single original series of cells forming the wall of the blastosphere. Hermann Foll's observations are of especial value, since he shows most carefully how, from the earliest period, even when the egg is unicellular, its central part has the character of the endodermal cells, its peripheral part that of the ectodermal cells.

The question now arises, can the *Gastrulæ* which arise by invagination be regarded as equivalent to those which arise by internal segregation of an endoderm from an ectoderm? and if so, which is the typical

* Annals and Mag. Nat. Hist. 1873, xi. p. 321.

† His most recent views on this matter are contained in a pamphlet dated June 7, 1873, 'Die Gastræa-Theorie.'

or ancestral mode of development, and what relation has the orifice of invagination in the one case to the mouth which, later, breaks its way through in the other?

It is not within the scope of the present memoir to discuss these questions at length; but the author is of opinion that we must regard the *Gastrula*-sac with its endoderm and ectoderm as strictly equivalent (homogenous, to use another expression) in the two sets of cases. One of the two methods is the typical or ancestral method of development, and the departure from it in the other cases is due to some disturbing condition. He believes that we shall be able to make out that disturbing element in the condition of the egg itself as laid, in the presence in that egg of a greater or less amount of the adventitious nutritive material which Edouard van Beneden calls "deutoplasm." This and certain relations of bulk in the early developed organs of the various embryos considered, determine the development either by invagination or by delamination. The relation of bulk to the process of invagination may be illustrated from a fact established in the preceding communications. In *Loligo* the large otocysts develop, each, by a well-marked invagination of the epiblast, forming a deep pit which becomes the cavity of the cyst. In *Aplysia* the smaller otocysts develop, each, by a simple vacuolation of the epiblast without invagination. In *Loligo* the chief nerve-ganglia develop by invagination of the epiblast, in *Aplysia* by simple thickening. Again, in Vertebrata the nerve-cord develops by a long invagination of the epiblast; in *Tubifex* and *Lumbricus* the corresponding nerve-cord develops by a thickening of the epiblast without any groove and canal of invagination.

The bulkier structures in these cases are seen to develop by invagination, the smaller by direct segregation. Invagination therefore acts as an economy of material, a hollow mass being produced instead of a solid mass of the same extent.

That the presence of a quantity of deutoplasmic matter, or of a partially assimilated mass of such matter, in the original egg is not accompanied by well-marked invagination of the blastosphere, while the absence of much deutoplasm is the invariable characteristic of eggs which develop a *Gastrula* by invagination, is shown by a comparison of *Aplysia* and *Loligo* with *Pisidium* and *Limax*, and of the Bird with the Batrachian. In some cases, such as Selenka has characterized by the term "epiboly," it seems that the enclosure of the large yelk-mass by the overgrowth of cleavage-cells may be held as equivalent to the invagination of the large yelk-cells by "emboly;" and the intermediate character which the development of *Euaxes* and *Lumbricus* present in this respect, as described by Kowalevsky, tends very strongly to establish a transition.

But the mode of development of the *Gastrula* of Geryonidæ, described with so much minuteness by Foll, which is obviously the same as that of the *Gastrulæ* of Spongiadæ and most Hydroids, is clearly no masked case

of invagination. There is no question of "epiboly" here, but a direct and simple splitting of one cell into two; so that what was a sac formed by a layer of cells one deep, becomes a sac formed by a layer of cells two deep, or of two layers each one deep.

It is yet a question for much further inquiry as to how this mode of forming a double-walled *Gastrula* can be derived from, or harmonized with, the formation of *Gastrulæ* by the embolic or epibolic forms of invagination.

It would certainly seem at present that the orifice of invagination of the invaginate *Gastrula* must *not* be regarded as the equivalent of the later erupting *mouth* of the segregate *Gastrula* *, which is the true permanent mouth of the Sponge or Cœlenterate. In no case is the orifice of invagination of the invaginate *Gastrula* known to persist under any form; it appears solely to effect the invagination, and when that is effected vanishes.

Enough has been said to show the importance of observations relating to the *Gastrula*-phase of development. In the paper well-marked invaginate *Gastrulæ* are described from:—

1. *Pisidium* (Lamellibranch).
2. *Tergipes* (Nudibranch).
3. *Polycera* (Nudibranch).
4. *Limax* (Pulmonate).
5. *Limnæus* (Pulmonate).

In addition to these cases of the development of invaginate *Gastrulæ* among Mollusca, the examination of the very beautiful figures in the papers of Lovén on molluscan development leaves no doubt that he has observed invaginate *Gastrulæ* in the following cases, but has not understood their structure:—

6. *Cardium* (Lamellibranch).
7. *Crenella* (Lamellibranch).

Similarly, Karl Vogt's observations on *Actæon* indicate the same state of things as the author has pointed out in *Polycera*; and hence we may add:—

8. *Actæon* (Nudibranch),

and, finally, from Kowalevsky's statement, though not accompanied by figure or description,

9. *Atalanta* (Heteropod).

The second matter of theoretical interest (namely, the early features in the development of the shell) has not been previously discussed, since the structures described in the paper as shell-patch, shell-groove, and shell-plug were unknown.

If, as seems justifiable, the Cephalopoda are to be regarded as more

* In his paper in the 'Annals' for May 1873 the author has inclined to the view that it *may* be so regarded.

nearly representing the molluscan type than do the other classes, or, in other words, more closely resemble the ancestral forms than they do, we might look, in the course of the development of the less typical Mollusca, for some indication of a representative of the internal pen of the higher Cephalopoda. We might expect to find some indication of the connexion between this and the calcareous shell of other forms; in fact the original shell of all Mollusca should be an internal one, or bear indications of a possible developmeut into that condition.

In *Pisidium*, in *Aplysia*, and in *Neritina* the author has submitted evidence of the existence of a specially differentiated patch of epidermic cells at the aboral pole, which develops a deep furrow, groove, or pit in its centre almost amounting to a sac-like cavity opening to the exterior. The first (chitinous) rudiment of the shell appears as a disk on the surface of this gland; but also, in some cases, the cavity or groove is filled by a chitinous plug.

Let the walls of the sac close and the activity of its lining cells continue, and we have the necessary conditions for the growth of such a "pen" as that of the Decapodous Cephalopods.

At present the details of the development of the "pen" in the Cephalopoda are not fully known; but the author has evidence that it is formed in an enclosed sac-like diverticulum of the epidermis, but he has not yet ascertained the earliest condition of this sac. The history of its development becomes surrounded with additional interest in relation to the shell-gland of the other Mollusca.

The position of the groove of the shell-gland in *Pisidium* suggests a possible connexion of its chitinous plug with the ligament, which it will be worth inquiring into in other developmental histories of Lamellibranchs.

The internal shells of other Mollusca besides the cuttlefish are certainly not in some cases (e. g. *Aplysia*) primitively internal, but become enclosed by overspreading folds of the mantle. But in the case of *Limax* and its allies, it is possible, though the matter requires renewed investigation, that the shell is a primitively internal one representing the shell-plug.

There is yet one more possible connexion of this shell-gland and plug: this is the chitinous secretion by which *Terebratula* and its allies fix themselves to rocks &c. The position of the peduncle exactly corresponds to that of the shell-gland; and an examination of Professor Morse's recently published account of the development of *Terebratulina* leaves little doubt that at the pole of attachment, which very early develops its function and fixes the embryo, an in-pushing occurs, and a kind of shallow gland is formed which gives rise to the horny cement. The author's own observations on the development of *Terebratula vitrea* do not extend to so early a period as this.

It is perhaps scarcely necessary, in conclusion, to point out the close resemblance of shell-gland and plug to the byssal gland and its secretion.

They are closely similar structures; but there does not appear to be any reason for considering them "serial homologues," or more closely related than are, say, the hairs on the head of a man with the hairs on his chest.

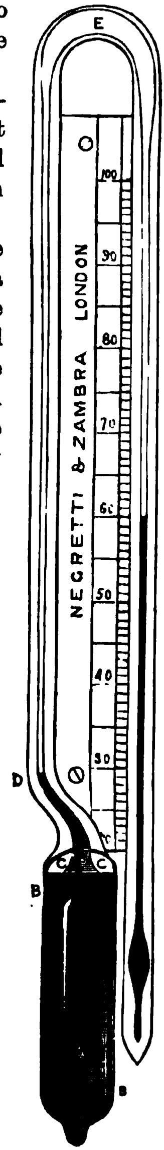
II. "On a New Deep-sea Thermometer." By Henry Negretti and Joseph Warren Zambra. Communicated by Dr. Carpenter, F.R.S. Received March 5, 1874.

The Fellows of the Royal Society are perfectly aware of the assistance afforded by Her Majesty's Government (at the request of the Royal Society) for the purpose of deep-sea investigations, and have been made acquainted with their results by the Reports of those investigations published in the 'Proceedings of the Royal Society' and by the interesting work of Professor Wyville Thomson. Among other subjects, that of the temperature of the sea at various depths, and on the bottom itself, is of the greatest importance. The Fellows are also aware that for this purpose a peculiar thermometer was and is used, having its bulb protected by an outer bulb or casing, in order that its indications may not be vitiated by the pressure of the water at various depths, that pressure being about 1 ton per square inch to every 800 fathoms. This thermometer, as regards the protection of the bulb and its non-liability to be affected by pressure, is all that can be desired; but unfortunately the only thermometer available for the purpose of registering temperature and bringing those indications to the surface is that which is commonly known as the Six's thermometer—an instrument acting by means of alcohol and mercury, and having movable indices with delicate springs of human hair tied to them. This form of instrument registers both maximum and minimum temperatures, and as an ordinary out-door thermometer it is very useful; but it is unsatisfactory for scientific purposes, and for the object which it is now used (viz. the determination of deep-sea temperatures) it leaves much to be desired. Thus the alcohol and mercury are liable to get mixed in travelling, or even by merely holding the instrument in a horizontal position; the indices also are liable either to slip if too free, or to stick if too tight. A sudden jerk or concussion will also cause the instrument to give erroneous readings by lowering the indices, if the blow be downwards, or by raising them, if the blow be upwards. Besides these drawbacks, the Six's thermometer causes the observer additional anxiety on the score of inaccuracy; for, although we get a *minimum* temperature, we are by no means sure of the point where this minimum lies. Thus Professor Wyville Thomson says ('Depths of the Sea,' p. 139):—"The determination of temperature has hitherto rested chiefly upon the registration of minimum thermometers. It is obvious that the temperature registered

by *minimum* thermometers sunk to the bottom of the sea, even if their registration were unaffected by the pressure, would only give the lowest temperature reached *somewhere* between top and bottom, not *necessarily* at the bottom itself. The temperatures at various depths might indeed (provided they nowhere increased on going deeper) be determined by a series of minimum thermometers placed at different distances along the line, though this would involve considerable difficulties. Still, the liability of the index to slip, and the probability that the indication of the thermometers would be affected by the great pressure to which they were exposed, rendered it very desirable to control their indications by an independent method." Again, at page 299, we find :—"I ought to mention that in taking the bottom temperature with the Six's thermometer the instrument simply indicates the lowest temperature to which it has been subjected; so that if the bottom water were warmer than any other stratum through which the thermometer had passed, the observations would be erroneous." Undoubtedly this would be the case in extreme latitudes, or in any spot where the temperature of the air is colder than that of the ocean. Certainly the instrument might be warmed previous to lowering; but if the coldest water should be on the surface, no reading, to be depended upon, could be obtained.

It was on reading these passages in the book above referred to that it became a matter of serious consideration with us whether a thermometer could be constructed which could not possibly be put out of order in travelling or by incautious handling, and which should be above suspicion and perfectly trustworthy in its indications. This was no very easy task. But the instrument now submitted to the Fellows of the Royal Society seems to us to fulfil the above onerous conditions, being constructed on a plan different from that of any other self-registering thermometers, and containing as it does nothing but mercury, neither alcohol, air, nor indices. Its construction is most novel, and may be said to overthrow our previous ideas of handling delicate instruments, inasmuch as its indications are only given by upsetting the instrument. Having said this much, it will not be very difficult to guess the action of the thermometer; for it is by upsetting or throwing out the mercury from the indicating column into a reservoir at a particular moment and in a particular spot that we obtain a correct reading of the temperature at that moment and in that spot. First of all it must be observed that this instrument has a protected bulb, in order to resist pressure. This protected bulb is on the principle devised by us some sixteen years since, when we supplied a considerable number of thermometers thus protected to the Meteorological Department of the Board of Trade; and they are described by the late Admiral FitzRoy in the first Number of the 'Meteorological Papers,' page 55, published July 5th, 1857. Referring to the erroneous readings of all thermometers, consequent on their delicate bulbs being compressed by the great pressure of the ocean, he

says:—"With a view to obviate this failing, Messrs. Negretti and Zambra undertook to make a case for the weak bulbs, which should transmit temperature, but resist pressure. Accordingly a tube of thick glass is sealed outside the delicate bulb, between which and the casing is a space all round, which is nearly filled with mercury. The small space not so filled is a vacuum, into which the mercury can be expanded, or forced by heat or mechanical compression, without doing injury to or even compressing the inner or much more delicate bulb."



The thermometers now in use in the 'Challenger' Expedition are on this principle, the only difference being that the protecting chamber has been partly filled with alcohol instead of with mercury; but that has nothing to do with the principle of the invention.

We have therefore a protected bulb thermometer, like a siphon with parallel legs, all in one piece, and having a continuous communication, as in the annexed figure. The scale of this thermometer is pivoted on a centre, and being attached in a perpendicular position to a simple apparatus (which will be presently described), is lowered to any depth that may be desired. In its descent the thermometer acts as an ordinary instrument, the mercury rising or falling according to the temperature of the stratum through which it passes; but so soon as the descent ceases, and a reverse motion is given to the line, so as to pull the thermometer to the surface, the instrument turns once on its centre, first bulb uppermost, and afterwards bulb downwards. This causes the mercury, which was in the left-hand column, first to pass into the dilated siphon bend at the top, and thence into the right-hand tube, where it remains, indicating on a graduated scale the exact temperature at the time it was turned over. The woodcut shows the position of the mercury *after* the instrument has been thus turned on its centre. A is the bulb; B the outer coating or protecting cylinder; C is the space of rarefied air, which is reduced if the outer casing be compressed; D is a small glass plug on the principle of our Patent Maximum Thermometer, which cuts off, in the moment of turning, the mercury in the column from that of the bulb in the tube, thereby ensuring that none but the mercury in the tube can be transferred into the indicating column; E is an enlargement made in the bend so as to enable the mercury to pass quickly from one tube to another in revolving; and F is the indicating tube, or thermometer proper. In its action, as soon as

the thermometer is put in motion, and immediately the tube has acquired a slightly oblique position, the mercury breaks off at the point D, runs into the curved and enlarged portion E, and eventually falls into the tube F, when this tube resumes its original perpendicular position.

The contrivance for turning the thermometer over may be described as a short length of wood or metal having attached to it a small rudder or fan; this fan is placed on a pivot in connexion with a second, and on this second pivot is fixed the thermometer. The fan or rudder points upwards in its descent through the water, and necessarily reverses its position in ascending. This simple motion or half turn of the rudder gives a whole turn to the thermometer, and has been found very effective.

Various other methods may be used for turning the thermometer, such as a simple pulley with a weight which might be released on touching the bottom, or a small vertical propeller which would revolve in passing through the water.

March 19, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Right Hon. Viscount Cardwell was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Notice of Experiments concerning the Chemical Constitution of Saline Solutions." By WALTER NOEL HARTLEY, F.C.S., Demonstrator of Chemistry, King's College, London. Communicated by Professor STOKES, Sec. R.S. Received February 3, 1874.

The author has been engaged in investigating the above subject during the last eighteen months, and his experiments being still in progress, he thinks it desirable to place the following observations on record.

In the examination of the absorption-spectra, as seen in wedge-shaped cells, of the principal salts of cerium, cobalt, copper, chromium, didymium, nickel, palladium, and uranium, to the number of nearly sixty different solutions, it was noticed that the properties of the substances in regard to changes of colour could be ascertained by noticing the absorption-curves and bands, so that, provided water be without chemical action, it could be foreseen what change would occur on dilution of a saturated solution.

The effect of Heat on Absorption-spectra.

When saturated solutions of coloured salts are heated to 100° C., 1st,

there are few cases in which no change is noticed. 2ndly, generally the amount of light transmitted is diminished to a small extent by some of the more refrangible, the less refrangible, or both kinds of rays being obstructed. 3rdly, there is frequently a complete difference in the nature of the transmitted light. Anhydrous salts not decomposed, hydrated compounds not dehydrated at 100° C., and salts which do not change colour on dehydration, give little or no alteration in their spectra when heated.

Solutions of hydrated salts, and most notably those of haloid compounds, do change; and the alteration is, if not identical with, similar to that produced by dehydration and the action of dehydrating liquids, such as alcohol, acids, and glycerine, on the salts in crystals or solution.

A particular instance of the action of heat on an aqueous solution is that of cobalt chloride, which gives a different series of dark bands in the red part of the spectrum at different temperatures, ranging between 23° C. and 73° C. Band after band of shadow intercepts the red rays as the temperature rises, till finally nothing but the blue are transmitted. Drawings of six different spectra of this remarkable nature have been made. The changes are most marked between 33° and 53°, when the temperature may be told almost to á degree by noting the appearance of the spectrum. Though to the unaided eye cobalt bromide appears to undergo the same change, yet, as seen with the spectroscope, it is not of so curious a character, the bands being not so numerous.

With cobalt iodide a band of red light is transmitted at low temperatures; the band of light moves towards the opposite end of the spectrum, with rise of temperature, until it is transferred to such a position that it consists of green rays only. In this instance the change to the eye is more striking when seen without the spectroscope, because the mixtures of red, yellow, and green rays, which are formed during the transition, give rise to very beautiful shades of brown and olive-green. Thus a saturated solution at 16° C. was of a brown colour, at $-10°$ C. it became of a fiery red and crystals separated, at $+10°$ reddish brown, at 20° the same, at 35° vandyke brown, at 45° a cold brown tint with a tinge of yellowish green, at 55° a decidedly yellowish green in thin layers and yellow-brown in thick, at 65° greenish brown, thin layers green, and at 75° olive-green. An examination of this cobalt salt has shown that there are two distinct crystalline hydrates—the one, formed at high temperatures, has the formula $CoCl_2,2H_2O$, and is of a dark green colour; the other, which contains a much larger proportion of crystalline water, $CoCl_2, 6H_2O$, is produced at a low temperature, and its colour is generally brown, in cold weather inclining to red.

The action of heat on solutions of didymium is characterized by a broadening of the black lines seen in the spectrum, more especially of the important band in the yellow; and in the case of potassio-didymium nitrate, this is accompanied by the formation of a new line. In the case

of didymium acetate, which decomposes with separation of a basic salt, the lines thickened on heating.

Thermo-chemical Experiments.

Regnauld (Institut, 1864; Jahresbericht, 1864, p. 99) has shown that on diluting a saturated solution of a salt, as a rule there is an absorption of heat; but in one or two cases he noticed that heat was evolved. The change in colour that takes place on the dilution of saturated solutions of cobalt iodide, cupric chloride, bromide, and acetate is very remarkable. There is every likelihood that this phenomenon is due in each case to the formation of a liquid hydrate. It is impossible of belief that accompanying such a circumstance there should be no measurable development of heat; and the author's experiments have proved that in the above cases, at any rate, the heat disengaged is very considerable—amounting, for instance, on the part of cupric chloride, at least to about 2565 units when 1 gram molecule of the crystalline salt is dissolved in its minimum of water at 16° C. and brought into contact with sufficient to make the addition of 40 Aq. These numbers only roughly approximate to the truth. On diluting a solution of cobalt iodide till the red colour appears, the thermal effect must be much greater, as not only does it register several degrees on an ordinary thermometer, but it may be perceived by the hand.

The conclusions indicated by these results are obvious, but it is beyond the scope of this paper to refer to them. The writer hopes before long to complete his experiments with the view of having them communicated to the Royal Society.

II. "Note on the Intracellular Development of Blood-corpuscles in Mammalia." By Edward Albert Schäfer. Communicated by Dr. Sharpey, V.P.R.S. Received January 22, 1874.

If the subcutaneous connective tissue of the new-born rat* is examined under the microscope in an indifferent fluid, it is found to consist chiefly of an almost homogeneous hyaline ground-substance, which is traversed by a few wavy fibres, and has a considerable number of exceedingly delicate, more or less flattened cells scattered throughout the tissue. The cells here spoken of are of course the connective-tissue corpuscles. They are not much branched as a rule (at any rate their branches do not extend far from the body of the corpuscle), and they are mainly distinguished by the extraordinary amount of vacuolation which they exhibit—by which is meant the formation within the protoplasm of minute clear spherules, less refractive than that substance, and probably, therefore, spaces in it containing a watery fluid. The nuclei, of which there is generally not more than one in each cell, are frequently obscured by the vacuoles, but, when visible, are seen to be round or oval in shape and

* The animal employed was the white rat.

beautifully clear and homogeneous; they commonly contain either one or two nucleoli. It is from these cells that the blood-vessels of the tissue are formed, and within them, red, and perhaps also, white blood-corpuscles become developed.

Of the vacuolated cells above described some possess a distinct reddish tinge, either pretty evenly diffused over the whole corpuscle, or in one or more patches, not distinctly circumscribed, but fading off into the surrounding protoplasm. Others contain either one, two, or a greater number of reddish globules, consisting apparently of hæmoglobin. These vary in size, from minute specks to spherules as large as, or even larger than, the red corpuscles of the adult: in cells which are apparently least developed it is common to find them of various sizes in the same cell; whereas cells which are further advanced in development are not uncommonly crowded with hæmoglobin-globules, tolerably equal in point of size, and differing from the adult corpuscle only in shape. It is important to remark that there is, at no time, an indication of any structure within the globules resembling a nucleus: the nucleus of the cell also appears, up to this point at least, to undergo no change. In fact the formation of the hæmoglobin-globules reminds one rather of a deposit within the cell-substance such as occurs in developing fat-cells, the difference being that in the latter case the deposited globules eventually run together into one drop, whereas in the former they remain distinct as they increase in size and eventually take on the flattened form.

Before, however, this change occurs in the hæmoglobin-globules, the cells containing them become lengthened, and are soon found each to contain a cavity, within which the globules now lie. This cavity is probably formed by a coalescence of the vacuoles of the cell, or, what amounts to the same thing, by the enlargement of one vacuole and the absorption of the rest into it. The cell now comes to resemble a segment of a capillary, but with pointed and closed extremities; it is of an elongated fusiform shape, and consists of a hyaline protoplasmic wall (in which the nucleus is imbedded) enclosing blood-corpuscles in a fluid—blood, in fact.

Two or more such cells may become united at their ends, a communication being established between their cavities; indeed, by aid of branches sent out from the sides a number of cells may unite to form a complete plexus of capillary vessels containing blood, and situate at a considerable distance in the tissue from any vessels in which blood is circulating. Eventually, however, these last become united with the newly developed capillaries, and the blood contained in the latter thus gets into the general circulation.

With regard to the mode of junction of the capillary-forming cells with one another, and with processes from preexisting capillaries, it has seemed to me to occur most commonly, not by a growing together of their extreme points, as commonly described, but rather by an overlapping and coaptation of their fusiform ends, which, at first solid, become subse-

quently hollowed by an extension into them of the cavity of the cell or capillary, the partition between the two being finally absorbed.

The best preparations for demonstrating the facts above described are obtained from the subcutaneous tissue of the upper part of the fore limb, and from that under the skin of the back—regions in which, in the adult rat, this tissue becomes almost entirely converted into fat. Even in the new-born animal some portions have already undergone this change; and it is principally in the neighbourhood of such patches that the hæmapoietic cells are met with. It is only when the young rats are not more than a few days old that the formation of blood-vessels is preceded by a development of blood-corpuscles within the same cells as form the vessels: in such other animals as I have hitherto examined this phenomenon seems to occur only whilst still in the fœtal state. The immature condition in which the young of the rat are brought forth is sufficient to account for this difference.

The observations here recorded as to the intracellular development of blood-corpuscles are in many respects in accordance with what has already been described by others as occurring in the *area vasculosa* and other parts of the embryo chick. It has not, however, appeared desirable to enter into the literature of the subject in this brief notice.

III. "On the Attractions of Magnets and Electric Conductors." By George Gore, F.R.S. Received January 27, 1874.

Being desirous of ascertaining whether, in the case of two parallel wires conveying electric currents, the attractions and repulsions were between the currents themselves or the substances conveying them, and believing this question had not been previously settled, I made the following experiment:—

I passed a powerful voltaic current through the thick copper wire of a large electromagnet, and then divided it equally between two vertical pieces of thin platinum wire of equal diameter and length (about six or seven centimetres), so as to make them equally white-hot, the two wires being attached to two horizontal cross wires of copper.

On approaching the two vertical wires symmetrically towards the vertical face of one pole of the horizontally placed magnet, and at equal distances from it, so that the two downward currents in them might be equally acted upon by the downward and upward portions respectively of the currents which circulated round the magnet-pole, the one was strongly bent towards and the other from the pole, as was, of course, expected; but not the least sign of alteration of relative temperature of the two wires could be perceived, thereby proving that not even a small proportion of the current was repulsed from the repelled wire or drawn into the attracted one, as would have occurred had the attraction and repulsion taken place, even to a moderate degree, between the currents

themselves; and I therefore conclude that *the attractions and repulsions of electric conductors are not exerted between the currents themselves, but between the substances conveying them.*

Some important consequences appear to flow from this conclusion, especially when it is considered in connexion with Ampère's theory of magnetism, and with the molecular changes produced in bodies generally by electric currents and by magnetism.

As every molecular disturbance produces an electric alteration in bodies, so, conversely, the discoveries of numerous investigators have shown that every electric current passing near or through a substance produces a molecular change, which is rendered manifest in all metals, liquid conductors, and even in the voltaic arc by the development of sounds, especially if the substances are under the influence of two currents at right angles to each other. In iron it is conspicuously shown also by electro-torsion, a phenomenon I have found and recently made known in a paper read before the Royal Society.

Numerous facts also support the conclusion that the molecular changes referred to last as long as the current. De la Rive has shown that a rod of iron, either transmitting or encircled by an electric current, emits, as long as the current lasts, a different sound when struck; and we know it also exhibits magnetism. The peculiar optical properties of glass and other bodies with regard to polarized light discovered by Faraday also continue as long as the current. A rod of iron also remains twisted as long as it transmits and is encircled by electric currents; and in steel and iron the molecular change (like magnetism) partly remains after the currents cease, and enables the bar to remain twisted.

That the peculiar molecular structure produced in bodies generally by the action of electric currents also possesses a definite direction with regard to that of the current, is shown by the rigidly definite direction of action of magnetized glass and many other transparent bodies upon polarized light, also by the difference of conductivity for heat and for electricity in a plate of iron parallel or transverse to electric currents, by the stratified character of electric discharges in rarefied gases and the action of electric currents upon it, and especially by the phenomena of electro-torsion. In the latest example an upward current produces a reverse direction of twist to a downward one, and a right-handed current develops an opposite torsion to a left-handed one; and the two latter are each internally different from the former. As each of these four torsions is an outward manifestation of the collective result of internal molecular disturbance and possesses different properties, these four cases prove the existence of four distinct molecular movements and four corresponding directions of structure; and the phenomena altogether are of the most rigidly definite character.

As an electric current imparts a definite direction of molecular structure to bodies, and as the attractions and repulsions of electric wires are

between the wires themselves and not between the currents, repulsion instead of attraction must be due to *difference of direction of structure* produced by difference of direction of the currents.

Although the Ampèrean theory has rendered immense service to magnetic science, and agrees admirably with all the phenomena of electromagnetic attraction, repulsion, and motion, it is in some respects defective; it assumes that magnetism is due to innumerable little electric currents continually circulating in one uniform direction round the molecules of the iron; but there is no known instance of electric currents being maintained without the consumption of power, and in magnets there is no source of power; electric currents also generate heat, but a magnet is not a heated body.

If, however, we substitute the view that the phenomena of attraction and repulsion of magnets are due, not to continuously circulating electric currents, but (as in electric wires) to definite directions of molecular structure, such as is shown by the phenomena of electro-torsion to really exist in them, the theory becomes more perfect. It would also agree with the fact that iron and steel have the power of retaining both magnetism and the electro-torsional state after the currents or other causes producing them have ceased.

According so this view, a magnet, like a spring, is not a source of power, but only an arrangement for storing it up, the power being retained by some internal disposition of its particles acting like a "ratchet" and termed "coercive power." The fact that a magnet becomes warm when its variations of magnetism are great and rapidly repeated, does not contradict this view, because we know it has then, like any other conductor of electricity, electric currents induced in it, and these develop heat by conduction-resistance.

According also to this view, any method which will produce the requisite direction of structure in a body will impart to it the capacity of being acted upon by a magnet; and any substance, ferruginous or not, which possesses that structure has that capacity; and, in accordance with this, we find that a crystal of cyanite (a silicate of alumina) possesses the property, whilst freely suspended, of pointing north and south by the directive influence of terrestrial magnetism, and one of stannite (oxide of tin) points east and west under the same conditions.

IV. "Spectroscopic Observations of the Sun." By J. Norman Lockyer, F.R.S., and G. M. Seabroke, F.R.A.S. Received February 2, 1874.

(Abstract.)

This paper consisted of the observations made of the sun's chromosphere and of the prominences for the period 1st September, 1872, to 31st December, 1873. Details are given of the modes of observation adopted.

March 26, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Organization of the Fossil Plants of the Coal-measures.—Part VI. Ferns." By W. C. Williamson, F.R.S., Professor of Natural History in Owens College, Manchester. Received March 18, 1874.

(Abstract.)

The author called attention to the various methods of classifying the fern-stems and petioles of the Coal-measures adopted by Cotta, Corda, Brongniart, and others, and to the difficulties which attend those methods. Some of those difficulties had been already felt and partially removed by M. Brongniart. All the generic distinctions hitherto adopted were based upon variations in the form, number, and arrangement of the vascular bundles. These elements vary so much, not only in different species of the same genus, but in different parts of the same petiole, as to make them most untrustworthy guides to generic distinctions. The consequence has been an enormous multiplication of genera; but, notwithstanding their number, the author found that if he adopted the methods of his predecessors he would have to establish additional ones for the reception of his new forms. Under these circumstances he decides that it will be better to include the entire series of these petioles, provisionally, under the common generic term of *Rachiopteris*. This plan dispenses with a number of meaningless genera, and is rendered additionally desirable by the circumstance that all the petioles to which these numerous generic names have been applied belong to fronds which have already received other names, such as *Pecopteris*, *Sphenopteris*, &c., only the structure of fronds found in the shales, and their respective petioles of which we have ascertained the structure, have not yet been correlated.

As a preparation for the present investigation, the author made an extensive series of researches amongst recent British and foreign fern-stems and petioles, with the object of ascertaining not only the modifications in their arrangements in different parts of the same plant, but especially of studying the modes in which secondary and tertiary vascular bundles were derived from the primary ones. This inquiry led him over the ground previously traversed by M. Trécul, and, so far as British ferns were concerned, by Mr. Church.

The most common general forms exhibited by transverse sections of

these bundles in recent petioles may be represented by the letters H, T, U, and X. As a general rule, the secondary bundles are given off from that part of the primary one which happens to be nearest to the secondary rachis to be supplied. Thus in some cases the upper arms of the X will merely be prolonged and their *ends* detached; in other cases a loop projects from the *side* of one or both arms of the U, and becomes detached as a ring.

The first petiole, described under the name of *Rachiopteris aspera*, is one in which transverse sections of the central vascular bundle exhibit modifications of the H form at its base, separating into two contiguous bundles higher up, and ultimately reverting to the V form—the gutter-shaped bundle (*en gouttière*) of M. Trécul. This is the plant to which, on a previous occasion, the author proposed to assign the generic name of *Edraxylon* (Proc. Roy. Soc. vol. xx. p. 438). The vessels are chiefly reticulate, with some of the barred and spiral types. The bark consists of a delicate inner parenchyma, the cubical cells of which are arranged vertically. This is enclosed in a coarser middle parenchyma, and the whole is surrounded by an outer layer, composed of intermingled parenchyma and prosenchyma, the latter being disposed in vertical fibrous bands, having wedge-shaped transverse sections, and being modifications of the sclerenchyma of authors. The outer surface of the bark is covered with innumerable little, obtuse, projecting cellular appendages, which are obviously abortive hairs. These appendages are relatively larger in the smaller rachis than in the larger petioles. In very young petioles transverse bands of small consolidated cells traverse the bark at numerous points, reminding us of the similar conditions seen in the *Heterangium Grievii*, described in a previous memoir. In the larger petioles these cellular bands have disappeared, and left in their places large intercellular lacunæ. Numerous fragments of the terminal rachis of the above plant have been obtained with the leaflets attached. For a long time the author believed that he could identify these with the detached leaflets of a *Pecopteris*, which are very abundant in the Oldham nodules; but later researches have led to the conclusion that the plant has been a *Sphenopteris*, closely allied to, if not identical with, the *S. Hoeninghausi* of Brongniart. The author proposes the provisional name of *Rachiopteris aspera* for the above plant.

The next petiole described is one to which Mr. Binney proposed ('Proceedings of the Literary and Philosophical Society of Manchester,' Jan. 9, 1872) to give the name of *Stauropteris Oldhamia*. This is one of the plants of which the vascular bundle, when seen in transverse section, exhibits the appearance of the letter X. The vessels composing this bundle are barred ones; they are sometimes grouped in four slightly coherent clusters, with some delicate, vertically elongated cells in or near their central point of conjunction. The same kind of cellular tissue surrounds the bundle, forming a thin layer, which passes rapidly into a very

thick layer, of coarse prosenchyma, and which has evidently been hard and woody, as in many of the recent Adiantiums. Towards the upper part of the petiole the vascular bundle becomes distinctly consolidated into a single cluster of crucial form; it then passes into a somewhat trifid form, and ultimately into a small cylindrical one. This petiole has branched much more freely than any of the others described. Two of the extremities of the crucial arms of the vascular bundle become first enlarged and then detached as two secondary bundles, which generally have an irregularly triangular transverse section, with long arms to the triangle. These triangular bundles are altogether different from the central axis of *Asterophyllites* described in a preceding memoir. The ultimate subdivisions of these secondary branches look more like the terminations of cylindrical rootlets than of petioles—which fact, combined with the circumstance that no traces of leaflets have been found associated with any of these ultimate twigs, renders the petiolar nature of this plant open to question, though the arguments in favour of its being a branching fern-petiole preponderate over those which militate against that conclusion. The author designates this plant *Rachiopteris Oldhamia*.

The next plant described is an exquisitely beautiful petiole from Burntisland, to two detached portions of which the author has already assigned the names of *Arpexylon duplex* and *A. simplex**, but which two forms he now proves to belong to the same plant. In the matured petiole the vascular bundle is always a double one. There is a central bundle, exhibiting a transverse section shaped like an hour-glass, one side of which is truncated and the other rounded, with a free, narrow, crescentic band at the more truncate of its enlarged extremities. At each of these extremities of the central bundle there is a longitudinal groove, which is shallow on the truncated side nearest to the crescentic bundle, but so surrounded by small vessels at the opposite convex side as often to become converted into a longitudinal canal. The hour-glass bundle always reappears in various specimens under the same aspect; but the crescentic one divides into two lateral halves, and the ends of each of these two subdivided parts curl under their more central portions. We thus obtain two of the crescentic structures previously designated *Arpexylon simplex*. These crescents are traced outwards through the bark to lateral secondary rachides. The vessels thus detached from the truncated side of the central hour-glass bundle now reappear at its opposite and more convex side, whence, in turn, they again become detached; so that the truncate surface with its crescentic appendage, and the more oblate one with its almost closed canal, have alternately reversed their positions in the petiole as each secondary rachis was given off. Alternating distichous tertiary rachides spring from these secondary ones.

Two plants which appear to be identical with those described by M. Renault, under the names of *Zygopteris Lacattii* and *Z. bibractiensis*, are next

* Proceedings of the Royal Society, vol. xx. p. 438.

examined*. In these plants the section of the central bundle exhibits a form of the letter H. The vessels of the large central transverse bar are all reticulated ones: the greater part of those of the terminal vertical bars are of the same character; but the outermost vessels of those latter structures are barred or quasi-scalariform. As in the case of *R. duplex*, already described, these outermost layers of barred vessels, accompanied by a few reticulated ones, become detached alternately from opposite sides of the H-shaped central bundle. Passing quickly through a thin delicate cellular inner bark, they enter the coarser parenchyma of a middle one, as two irregular clusters of vessels with one common investment prolonged from the innermost bark. On reaching the outer bark they become two distinct cylindrical bundles, each with its own delicate cortical investing layer; and thus invested, they emerge from the primary petiole to supply the secondary rachis.

The Oldham specimens of *Rachiopteris bibractiensis* agree with those described by M. Renault in having all their vessels of the barred type. The outer bark projects at numerous points in large conical abortive hairs, which almost assume a spinous aspect.

The author further figures and describes the section of a vascular axis, with a central cellular medulla surrounded by five contiguous crescentic masses of vascular tissue, whose concavities are directed outwards. This plant appears identical with the *Anarchopteris Decaisnii* of Renault.

II. "On the Motions of some of the Nebulæ towards or from the Earth." By William Huggins, D.C.L., LL.D., F.R.S. Received January 26, 1874.

The observations on the motions of some of the stars towards and from the earth which I had the honour to present to the Royal Society in 1872 appeared to show, from the position in the heavens of the approaching and receding stars, as well as from the relative velocities of their approach and recession, that the sun's motion in space could not be regarded as the sole cause of these motions. "There can be little doubt but that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars, and also a motion peculiar to each star"†.

It presented itself to me as a matter of some importance to endeavour to extend this inquiry to the nebulæ, as it seemed possible that some light might be thrown on the cosmical relations of the gaseous nebulæ to the stars and to our stellar system by observations of their motions of recession and of approach.

Since the date of the paper to which I have referred, I have availed

* Annales des Sciences Naturelles, 5e série, Bot. tome xii.

† Proceedings of the Royal Society, vol. xx. p. 392.

myself of the nights sufficiently fine (unusually few even for our unfavourable climate) to make observations on this point.

The inquiry was found to be one of great difficulty, from the faintness of the objects and the very minute alteration in position in the spectrum which had to be observed.

At first the inquiry appeared hopeless, from the circumstance that the brightest line in the nebular spectrum is not sufficiently coincident in character and position with the brightest line in the spectrum of nitrogen to permit this line to be used as a fiducial line of comparison. The line in the spectrum of the nebulæ is narrow and defined, while the line of nitrogen is double, and each component is nebulous and broader than the line of the nebulæ. The nebular line is apparently coincident with the middle of the less refrangible line of the double line of nitrogen*.

The third and fourth lines of the nebular spectrum are undoubtedly those of hydrogen; but their great faintness makes it impossible to use them as lines of comparison under the necessary conditions of great dispersive power, except in the case of the brightest nebulæ.

The second line, as I showed in the paper to which I have referred, is sensibly coincident with an iron line, wave-length 495·7; but this line is inconveniently faint, except in the brightest nebulæ.

In the course of some other experiments my attention was directed to a line in the spectrum of lead which falls upon the less refrangible of the components of the double line of nitrogen. This line appeared to meet the requirements of the case, as it is narrow, of a width corresponding to the slit, defined at both edges, and in the position in the spectrum of the brightest of the lines of the nebulæ.

In December 1872 I compared this line directly with the first line in the spectrum of the Great Nebula in Orion. I was delighted to find this line sufficiently coincident in position to serve as a fiducial line of comparison.

I am not prepared to say that the coincidence is perfect; on the contrary, I believe that if greater prism-power could be brought to bear upon the nebulæ, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebulæ.

The spectroscope employed in these observations contains two compound prisms, each giving a dispersion of 9° 6′ from A to H. A magnifying-power of 16 diameters was used.

In the simultaneous observation of the two lines it was found that if the lead line was made rather less bright than the nebular line, the small excess of apparent breadth of this latter line, from its greater brightness, appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible sides of the two lines appeared to be in a straight line across the spectrum. This line could be

* Proceedings of the Royal Society, vol. xx. p. 380.

therefore conveniently employed as a fiducial line in the observations I had in view.

In my own map of the spectrum of lead this line is not given. In Thalén's map (1868) the line is represented by a short line to show that, under the conditions of spark under which Thalén observed, this line was emitted by those portions only of the vapour of lead which are close to the electrodes.

I find that by alterations of the character of the spark this line becomes long, and reaches from electrode to electrode. As some of those conditions (such as the absence of the Leyden jars, or the close approximation of the electrodes when the Leyden jars are in circuit) are those in which the lines of nitrogen of the air in which the spark is taken are faint or absent, the circumstance of the line becoming bright and long or faint and short, inversely as the line of nitrogen, suggested to me the possibility that the line might be due not to the vapour of lead, but to some combination of nitrogen under the presence of lead vapour. As, however, this line is bright under similar conditions when the spark is taken in a current of hydrogen, this supposition cannot be correct.

A condition of the spark may be obtained in which the strongest lines of the ordinary lead spectrum are scarcely visible, and the line under consideration becomes the strongest in the spectrum, with the exception of the bright line in the extreme violet.

I need scarcely remark that the circumstance of making use of this line for the purpose of a standard line of comparison is not to be taken as affording any evidence in favour of the existence of lead in the nebulæ.

Each nebula was observed on several nights, so that the whole observing time of the past year was devoted to this inquiry. In no instance was any change of relative position of the nebular line and the lead line detected.

It follows that none of the nebulæ observed shows a motion of translation so great as 25 miles per second, including the earth's motion at the time. This motion must be considered in the results to be drawn from the observations; for if the earth's motion be, say, 10 miles per second from the nebula, then the nebula would not be receding with a velocity greater than 15 miles per second; but the nebula might be approaching with velocity as great as 35 miles per second, because 10 miles of this velocity would be destroyed by the earth's motion in the contrary direction.

The observations seem to show that the gaseous nebulæ as a class have not proper motions so great as the bright stars. It may be remarked that two other kinds of motion may exist in the nebulæ, and, if sufficiently rapid, may be detected by the spectroscope:—1. A motion of rotation in the planetary nebulæ, which might be discovered by placing the slit of the instrument on opposite limbs of the nebulæ. 2. A motion

of translation in the visual direction of some portions of the nebulous matter within the nebula, which might be found by comparing the different parts of a large and bright nebula.

Sir William Herschel states that "nebulæ were generally detected in certain directions rather than in others, that the spaces preceding them were generally quite deprived of stars, that the nebulæ appeared some time after among stars of a certain considerable size and but seldom among very small stars, that when I came to one nebula I found several more in the same neighbourhood, and afterwards a considerable time passed before I came to another parcel"*.

Since the existence of real nebulæ has been established by the use of the spectroscope, Mr. Proctor† and Professor D'Arrest‡ have called attention to the relation of position which the gaseous nebulæ hold to the Milky Way and the sidereal system.

It was with the hope of adding to our information on this point that these observations of the motions of the nebulæ were undertaken.

In the following list the numbers are taken from Sir J. Herschel's 'General Catalogue of Nebulæ.' The earth's motion given is the mean of the motions of the different days of observation.

No.	h.	H.	Others.	Earth's motion from Nebula.
1179	360	. ..	M. 42	7 miles per second.
4234	1970	..	Σ. 5	12 " "
4373	..	IV. 37.	..	1 " "
4390	2000	..	Σ. 6	2 " "
4447	2023	..	M. 57	3 " "
4510	2047	IV. 51.	..	14 " "
4964	2241	IV. 18.	..	13 " "

III. "On the Annual Variation of the Magnetic Declination." By J. A. Broun, F.R.S. Received February 11, 1874.

The first observations which seemed to show that the mean position of the declination-needle followed an annual law were those of Cassini, made, more than eighty years ago, in the hall of the Paris Observatory and in the *caves* below it (90 feet under ground). It cannot be said, however, that Cassini's result has been confirmed by subsequent observations, either as regards the direction or amounts of movement from month to month.

The extensive series of observations made in different parts of the

* Philosophical Transactions, 1784, p. 448.
† Other Worlds than Ours, pp. 280–290.
‡ Astronomische Nachrichten, No. 1906, p. 190.

world in modern times have given results so different that we must conclude either that the magnetic needle obeys different annual laws at each place, or that the differences are due to instrumental errors. The consequence has been that, after long, laborious, and expensive researches, it is still a question whether the magnetic needle obeys an annual law or not.

The results obtained at some observatories have made it very probable that, if an annual law exist, the range of the oscillation must be very small. It is therefore essential, in questioning any series of observations for this law, to be assured that the errors (instrumental or others) are neither considerable nor systematic.

I have concluded, from several series of observations made with suspension-threads bearing unmagnetic or slightly magnetic weights, that the systematic errors due to varying temperature or humidity are very small when the suspension-threads are carefully constructed with fibres from which the original torsion has been removed. Dr. Lloyd has concluded that threads with fibres differently twisted may produce comparatively large annual variations in different directions, according to the direction of the twist. There is little doubt, however, that the greatest errors are due to the unequal stretching and rupture of the different fibres which form the suspension-thread.

When the instrumental errors may be so considerable compared with the variations to be observed, it cannot be supposed extraordinary that instruments in different places give different results; and it appears essential so to eliminate the sources of error that two instruments in the same place may tell the same story before we attempt to announce the existence of any law.

If at sea two or more chronometers are necessary in case one may be affected by error, it seems not less necessary in scientific researches requiring continuous observations for years, where errors are so difficult of detection and elimination, that two or more instruments should be observed. These considerations induced me to establish at Trevandrum two declination-magnetometers of different construction, placed under considerably different atmospheric conditions; and it is to the results of sixteen years' comparative observation from these two instruments that I desire to draw the attention of physicists.

Both instruments had suspension-threads made with the utmost care. One, Dr. Lloyd's instrument, made by Mr. Grubb, of Dublin, with a magnet weighing nearly a pound, was placed in the large room of the Trevandrum Magnetic Observatory, which was always more or less open to the external air; and, although covered by a cotton-wadded hood and a series of boxes, it was much more liable to any errors due to atmospheric actions than the other. Its chief source of error was, however, connected with small movements of the telescope wire, although that was made to coincide, at varying intervals of time, with the transit-mark five miles distant.

The second instrument, made according to my own designs by Mr. P. Adie, of London, had a magnet weighing only about one sixth of the other; it was suspended under a glass bell from which the air was exhausted, and which was covered with two hoods—one with gilt surfaces, the other with cotton wadding. This instrument was placed in a closed room without windows or external openings, and with a terraced ceiling below the observatory roof. Observed from without (within the large room of the observatory), the diurnal variations of temperature in the instrument were not more than three tenths (0·3) of a degree Fahrenheit, while the annual variation was under 5° Fahr.*

The compared mean positions of the two magnets for each day, derived from hourly observations of both instruments during eleven years, and from eight daily observations during the remaining five years, will be found with all other details in the volume referred to in the note to the preceding paragraph. It will be sufficient for the purposes now in view to give here the chief conclusions from these means.

The monthly mean declinations having been freed from the secular movement, and the means for three groups of years having been taken, these means are represented very nearly by the following equations of sines ($\theta=0$, Jan. 15):—

Years.		
1854 to 1859	Adie.	$y=0'{\cdot}033 \sin(\theta+135°)+0'{\cdot}069 \sin(2\theta+299°)$.
	Grubb.	$y=0'{\cdot}030 \sin(\theta+150°)+0'{\cdot}078 \sin(2\theta+300°)$.
1860 to 1864	Adie.	$y=0'{\cdot}190 \sin(\theta+178°)+0'{\cdot}070 \sin(2\theta+324°)$.
	Grubb.	$y=0'{\cdot}099 \sin(\theta+211°)+0'{\cdot}062 \sin(2\theta+319°)$.
1865 to 1869	Adie.	$y=0'{\cdot}171 \sin(\theta+181°)+0'{\cdot}104 \sin(2\theta+342°)$.
	Grubb.	$y=0'{\cdot}062 \sin(\theta+228°)+0'{\cdot}122 \sin(2\theta+322°)$.

In the years 1854 to 1859 the movements of Grubb's telescope were very small, the daily mean declinations from both instruments differing rarely more than 0'·1 throughout the whole six years. It will be seen that the equations for these years agree very nearly. In spite of the greater movements of the telescope in following years (affecting chiefly the coefficient of $\sin\theta$), the epochs of maxima and minima derived from the two instruments differ but little, and all the principal deviations from the *mean* law for *any* year are confirmed by both instruments.

When the means for the whole sixteen years are taken, and the equi-

* Experiments with suspension-threads carrying slightly magnetic weights of nearly one pound, showed that the effect of a change of 1° Fahr. on the position of Grubb's magnet amounted to about 0'·003 (=0''·18)—a result deduced from the changes of temperature from hour to hour, as well as from those from day to day. I must refer to the first volume of the 'Trevandrum Observations,' now in the press, for the details of these experiments.

valent equations of sines are carried to four terms, the following results are obtained:—

1854 to 1869.

Adie. $y = 0'{\cdot}120 \sin(\theta + 175°) + 0'{\cdot}076 \sin(2\theta + 323°)$
$+ 0'{\cdot}011 \sin(3\theta + 299°) + 0'{\cdot}022 \sin(4\theta + 181°)$.

Grubb. $y = 0'{\cdot}056 \sin(\theta + 209°) + 0'{\cdot}095 \sin(2\theta + 315°)$
$+ 0'{\cdot}012 \sin(3\theta + 293°) + 0'{\cdot}022 \sin(4\theta + 197°)$.

From these equations we deduce the following epochs of maxima and minima:—

	Minima.	Maxima.
Adie.	January 26 and May 19.	March 14 and October 1.
Grubb.	January 13 and May 23.	March 18 and September 29.

The confirmation of the results from Adie's instrument by those from Grubb's, in spite of the errors of the latter, is so marked in each year and group of years, that we can affirm that at Trevandrum, in the south magnetic hemisphere, the magnetic needle obeys an annual law producing a double oscillation, having a minimum towards the end of May, the principal maximum near the end of September, another minimum in January, and a secondary maximum in the middle of March. Or, taking the results from Adie's instrument as most free from all error, the principal minimum occurs about a month before the June solstice, and the secondary minimum about a month after the December solstice; while the principal maximum occurs about a week after the September equinox, and the secondary maximum about a week before the March equinox.

In the result obtained by me from four years' observations (1843 to 1846) at Makerstoun, in Scotland, the greatest easterly position was attained in the end of April or beginning of May, and the greatest westerly (or least easterly) position in September. If that result, derived from a single instrument, can be accepted*, it would appear that the movements of the north end of the needle, in the annual variations, are in opposite directions at Trevandrum and Makerstoun at the same period of the year. This result agrees with that which I have found for the decennial inequality, or that in the south magnetic hemisphere; the law for the south end of the magnet is the same as that for the north end of the magnet in the north magnetic hemisphere: but it is opposed to the result obtained by me for the twenty-six day period, in which the easterly and northerly magnetic forces have their maxima at the same time in both hemispheres.

It follows that the results which are connected with the sun's rotation on its axis are the same in both hemispheres, while those related to

* I have always considered this result a near approximation to the truth, but it was not confirmed by the very limited series of observations made in the three subsequent years, years of great disturbance.

the earth's revolution round the sun appear opposite in the two hemispheres.

It might be supposed, as was done for the diurnal variation of magnetic declination, that the directions of motion being opposite in the two hemispheres, the amount of motion should diminish, and perhaps altogether disappear, at the magnetic equator. This does not seem to be the case for the annual law more than for the diurnal law, the range of the mean oscillation from four years' observations at Makerstoun being about 1'·0, which is little different from that found for Trevandrum (0'·33), the difference of the directive forces being considered.

The Society then adjourned over the Easter Recess to Thursday, April 16th.

Presents received March 5, 1874.

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"On the Nervous System of *Actinia*."—Part I. By Professor P. MARTIN DUNCAN, M.B. Lond., F.R.S., &c. Received October 9, 1873.

I. *A Notice of the Investigations of Homard, Haime, Schneider and Rötteken, and others on the subject.*

MM. Milne-Edwards and Jules Haime* wrote as follows in 1857 concerning the nervous attributes of the group of Cœlenterata called Zoantharia:—"They (les Coralliaires) enjoy a highly developed sensibility; not only do they contract forcibly upon the slightest touch, but they are, moreover, not insensible to the influence of light. Nevertheless, neither a nervous system nor organs of special sense have been discovered in them. It is true that Spix described and figured ganglions and nervous cords in the pedal disk of Actiniæ; but the observations of this naturalist, so far as the polypes are concerned, are not entitled to the least confidence.

"Some naturalists have supposed that the 'bourses calicinales' of the Actiniæ are eyes, and M. Huschke believes that certain capsules in the trunk of *Veretilla*, which contain calcareous bodies, are the organs of hearing. But these hypotheses do not rest upon any proved facts."

* Hist. Nat. des Coralliaires, vol. i. p. 11.

In 1864 Huxley noticed that, with regard to the Cœlenterata, "a nervous system has at present been clearly made out only in the Ctenophora"*.

Homard †, an admirable observer, contributed to the histology of the Actinozoa in 1851. He corrected Erdl's mistake concerning the supposed striation of the muscular fibrillæ of the tentacles, and also Quatrefage and Leuckart's notion concerning the rupture of the tentacular ends previously to the passage of water from them. Giving very good illustrations, he proved himself to be a very reliable investigator.

Amongst other parts of the Actinozoa, he paid especial attention to the minute anatomy of the "bourses calicinales." These bead-like appendages, situated just without the tentacles in some genera, but not in all, are also called chromatophores and "bourses marginales;" and their beautiful turquoise colour had rendered them attractive to previous anatomists, who had, as has already been noticed, guessed concerning their function.

Homard determined that they were folded elements of the skin in which the capsules (nematocysts) were enormously developed. He stated that the thread of these gigantic nematocysts was seen with difficulty. He noticed the transparency of some large cells in the bourses, and stated that, in his opinion, there was "some physiological relation between these little organs and the light."

Jules Haime (probably in 1855) examined the minute anatomy of *Actinia mesembryanthemum*, and his colleague, Milne-Edwards, quotes him in the 'Hist. Nat. des Coralliaires,' vol. i. p. 240. The lamented young naturalist found out that the chromatophores bore, so far as their number is concerned, a decided numerical relation with the number of the tentacles. He decided that they contained but few muscular fibres, and had navicular-shaped nematocysts, "diversement contournés," with indistinct threads within them. However, he recognized large transparent cells and pigmentary granules in them. The nematocysts of the chromatophores are larger than those of the tentacles. He was evidently not satisfied with the data upon which these coloured masses were decided to be of importance as organs of special sense. In all probability Haime was aware of Homard's work.

Kölliker and the German histologists added about this time, and later, to the exact knowledge respecting the histology of the muscles, skin, endothelium, and tentacular apparatus, but no advance was made towards the discovery of a nervous system in the *Actinia* for many years.

In 1871 the popular idea of the extent of the nervous system in

* Huxley, 'Elements of Comparative Anatomy,' p. 82. See Dr. Grant, F.R.S. &c., on *Beroë pileus*, Zool. Trans. vol. i. p. 10. See also 'A Manual of the Subkingdom Cœlenterata,' by J. R. Greene, 1861, p. 165.

† "Sur les Actiniæ," Ann. des Sciences Nat. 1851.

‡ Sea-side Studies, Eliz. and A. Agassiz, 1871, p. 12.

Actinia was expressed by Alex. Agassiz ‡, who wrote:—"Notwithstanding its extraordinary sensitiveness, the organs of the senses in the *Actinia* are very inferior, consisting only of a few pigment-cells accumulated at the base of the tentacles."

But in this year a great advance was made towards discovery by Profs. A. Schneider and Rötteken*. The first-named naturalist paid especial attention to the development of the lamellæ and septa in Corals and Actiniæ, and his colleague laboured in the histology of *Actinia* especially.

Working at a very great disadvantage, with specimens which had been preserved in alcohol, Rötteken produced a series of researches which added greatly to the knowledge already granted to science by Homard and Haime. So far as they bear on the nervous system, the result of his researches may be stated as follows:—"The bourses marginales" (chromatophores) are undoubtedly organs of sense, and, indeed, compound eyes. They are pyriform diverticula of the body-wall, standing between the tentacles and the outer margin of the peristome; they are constructed after the fashion of a retina, and the following layers of structure may be distinguished in them:—1, externally a cuticular layer broken up into "bacilli" by numerous pore-canals; 2, a layer of strongly refractile spherules, which may be regarded as lenses; 3, cones—hollow, strongly refractile, transversely striated cylinders or prisms rounded at the ends; these have hitherto been confounded with urticating capsules (nematocysts): at the exterior end of each cone there is generally one lens, and sometimes two or three may stand in the interspaces; 4, a granular fibrous layer occupying the interspaces between the cones; 5, a layer which is deeply stained by carmine, and contains numerous extremely fine fibres and spindle-shaped cells, probably nerve-fibres and cells; 6, the muscular layer; 7, the endothelium, which bounds the perigastric cavity.

Actinia mesembryanthemum was the species examined, and the diagram (Pl. II. fig. 15) will explain the relative position of the layers.

Rötteken could not determine the position of the pigment of the chromatophores from the alcoholized specimens. An examination of the minute anatomy of the tentacles of *Actinia cereus*, Ellis and Solander, determined that the refractile spherules and large cones were to be found on the tips of these organs.

Dana †, in his popular work on Corals and Coral Islands, appears to accept the statements quoted above. He states that "they sometimes possess rudimentary eyes;" and elsewhere, "they have crystalline lenses and a short optic nerve." He then observes:—"Yet Actiniæ are not

* Sitzungsbericht der Oberhessischen Gesellschaft für Natur- und Heilkunde, March 1871 (On the Structure of Actiniæ and Corals). Translated for the Ann. and Mag. of Nat. Hist. 1871, vii. p. 437, by W. S. Dallas, F.L.S. &c.

† Corals and Coral Islands, by James D. Dana, LL.D., 1872, pp. 41, 39.

known to have a proper nervous system; their optic nerves, where they exist, are apparently isolated, and not connected with a nervous ring such as exists in the higher Radiate animals."

II. *A Description of the Morphology of the Chromatophores.*

During the summer of 1871 the author of this communication was examining into the minute anatomy of *Actinia mesembryanthemum*, and had the advantage of possessing living specimens. Having satisfied himself of the general correctness of Rötteken's admirable work, he relinquished the inquiry until 1873, when he resumed it.

Every one who has endeavoured to anatomize one of the Actiniæ must acknowledge the excessive difficulties which accompany the attempt. The irritability of the muscular tissues, their persistent contraction during manipulation, the confusion caused by the abundance of different cellular histological elements, and the general sliminess of the whole, render the minute examination very troublesome and usually very unsatisfactory. Reagents are useful for rough examinations; but when the most delicate of the tissues are to be examined they must be floated under sea-water, and this must be the medium in which they must be examined under the microscope. Carmine-solution, osmic acid, and spirits of wine in weak solutions are useful after the natural appearances have been determined, but they exaggerate some histological elements and destroy others.

Great care must be taken in making the thin sections, and no tearing must be allowed; for it is of paramount importance, in endeavouring to trace the nervous system, that the relative position of parts should be retained.

It is useless to rely on any observations made with object-glasses lower than $\frac{1}{16}$-inch focus (immersive).

In examining the chromatophores, Actiniæ with very bright-coloured ones, and other specimens with these organs dull in tint, should be selected. Fresh subjects should be obtained, and it is not necessary to kill them first of all. The blades of very delicate scissors should be allowed to touch the desired chromatophore close to its base, and then as the *Actinia* commences to contract, they should be brought together gently and without wrenching the tissues. By this method the chromatophore will remain on the blades. Two or three chromatophores may be removed, with their intermediate tissues, without injury to the animal; but, of course, the excision must not be too deep, or the endothelium will be cut into.

A dropping-tube should be used to wash the chromatophore off the blades on to a glass slide, where a drop of sea-water awaits it.

Sections are by no means easy to make, but they are best performed under a power of 10 diameters with fine scalpels. The forceps must not be employed, as it crushes the tissues. If possible, very slight pressure

should be exercised on the thin glass, which is to be placed very carefully and wet over the object. After the examination, carmine should be added, or osmic-acid solution, 1 per cent. in strength; but no results can be relied on which are derived from the examination under the influence of reagents alone, as they modify the natural appearance greatly.

So far as the chromatophores are concerned, my investigations took the following course:—1. Rötteken's researches on the alcoholized *Actinia* were followed in recent specimens. 2. The tissues of the chromatophores, of their margins, and of the spaces between them were examined in a large specimen of a living pale-green variety of *Actinia mesembryanthemum* from the Mediteranean. 3. The tissues of the chromatophores of the *Actinia mesembryanthemum* were again examined with a view to explain the differences between M. Rötteken's and my own results.

The rounded, free, coloured, external layer of a chromatophore was carefully disengaged from the granular tissue beneath it, so that the bacilli of Rötteken, the refractile corpuscles, and his so-called cones were separated from the rest. This turquoise-coloured film was floated and carefully placed on a glass slide, the bacillary layer being inferior and on the glass, whilst the proximal ends of the cones were free in the water. No thin glass was placed over the film, and an object-glass of ½-inch focus was used. The appearance presented under this low power (by transmitted light) was very remarkable, for a great number of brilliant points of light were seen surrounded and separated by dark opaque tissue. When a ¼-inch object-glass was used, the appearance was less striking, for the points of light were more diffused. No trace of an object could be seen through the refractile tissues.

The transparent and refractile tissues were the so-called bacilli, the globular bodies and the "cones" already noticed; and the tissue, which was impermeable by light, consisted of the colouring-matter in small dull granules, cells small and round in outline and granular, and also the cell-walls of the cones.

Sections through a chromatophore were made at right angles to the point of the greatest convexity of the surface, and thin slices were floated off carefully from the line of section on to glass slides. The slices included (*a*) the coloured outside of the chromatophore, (*b*) the tissue beneath it, and (*c*) some muscular fibres which limit the endothelium. Sea-water was used as the medium, and a thin glass cover was applied after the specimens had been examined with a low power.

Externally was the baccillary layer (Pl. II. fig. 15). Rötteken describes this as a cuticular layer broken up into bacilli by numerous pore-canals. Examined, however, in the fresh subject, this external layer consisted of a vast multitude of small rod-shaped bodies, sharply rounded but conical at both ends, very transparent, and resembling the smallest

nematocysts of the tentacles without the internal thread (Pl. II. fig. 2). These are placed side by side, and the external rounded end of each is separated by a small space from the terminations of its neighbours. These ends are free and are in contact with the water in which the *Actinia* lives. The rods are cylinders, and are separated from each other by a very delicate film of protoplasm, in which are numerous dark opaque granules and a few flat simple colourless rounded cells (Pl. II. fig. 3). The inner ends are shaped like the external, and are embedded in the next layer of tissue. Each of these bodies is a simple cell filled with a transparent fluid. When a thin film of the surface of a chromatophore is removed and examined under a $\frac{1}{18}$-inch, the bacilli may be observed to crowd together over a layer of large refractile cells. The thin glass cover is generally sufficient to crush down the bacilli, so that their sides may be seen as they rest in all kinds of positions on the deeper cellular layer (Pl. II. fig. 17).

The bacilli are not found universally over the chromatophores, nor do they invariably cover the layer of large refractile globular cells.

It will be noticed, on examining excised portions which include two or three chromatophores and their intermediate tissue, that not only are they marked on their surface by foldings of their superficial tissue, but that between them there are others which are microscopic. These last rarely have bacilli. Moreover, in some parts of the margins of the chromatophores, other pigments are visible than the turquoise, and the red often predominates; the bacilli are not usually present there.

Beneath the superficial layer of bacilli and their separating protoplasm, which is faintly granular, there is some granular tissue with a few small spherical cells containing granules, and the inner ends of the bacilli are embedded therein (Pl. II. fig. 3).

This granular tissue is very thin, but it covers and dips down between the large refractile cells, which form the next layer (Pl. II. figs. 4, 13, 15, 16, 17).

These cells are more or less spherical; the cell-wall is very thin, and the contents are transparent, colourless, and refractile. Some have a pale grey tint, and one or more extremely faint nuclei are attached to the inner surface of the cell-wall. The ovoid shape is occasionally seen.

These large cells, which transmit light so readily, are universally found on the chromatophores; and when there are bacilli upon them, the spherical shape is common.

At the margins of the chromatophores, and where the red pigment commences, these refractile cells assume much larger dimensions and more irregular shapes. These refractile cells are, as has already been noticed, embedded in a tissue of granular and slightly cellular protoplasm, and this occasionally is differentiated into some peculiar structures.

Where there are no bacilli this granular tissue is increased in thickness

and becomes superficial; moreover the granules then contribute to the colour of the chromatophore, and probably they always do so to a certain degree.

The refractile cells are not invariably confined to the layer above the so-called cones of Rötteken, although they are often thus limited in their position, especially if there are bacilli covering them. In parts of the same chromatophore, where this apparently normal arrangement is seen, and especially on the microscopic chromatophores between the larger kinds, the large refractile spherules are found between and in the midst of groups of the cones (Pl. II. fig. 16).

In the chromatophores there is considerable variety in the size of the refractile cells; they appear to be developed from the small cells with a circular outline, which contain a few dark granules, and which are found in considerable abundance amidst the enveloping granular tissue (Pl. II. fig. 8).

The most striking of all the histological elements of the chromatophores are the cones of Rötteken, or the nematocysts with imperfectly visible threads of Homard. They are divisible into three series :—

α. Elongated simple cells, cylindrical in shape, with rounded and somewhat pointed extremities, consisting of a tough cell-wall which is capable of being bent without being broken or ruptured, and of colourless transparent contents which are rather viscid (Pl. II. fig. 5). They are four or five times the length of the bacilli, and three times their width. The cell-wall is faintly tinted with the peculiar colour of the chromatophore. These elongated cells are not conical, nor can they be really termed cones with any propriety; when observed through their greatest length, or when the light traverses their long axis, the cell-wall appears dark and the centre very refractile. They exist in vast multitudes over most parts of the chromatophore, and also in the intermediate tissue and its microscopic chromatophores.

β. Cells of the same shape as "*α*," but the cell-wall is faintly striated, the appearance being very distinct under a power of 2000 diameters (Pl. II. fig. 6). These cells are very numerous, and were noticed by Rötteken; they appear in the same position, and often amongst the cells with simple walls.

γ. Cells of the same shape and size as "*α* and *β*," with a well-developed thread within them, which usually has no barb (Pl. II. fig. 7).

These cells are common where there are no bacilli, but they occur here and there in all parts of the chromatophore circle.

In some rare instances the "Rötteken bodies" (for thus I would name these remarkable cells) are closely approximated, side by side, without the intervention of any structure; but, usually, there is a very thin layer of granular protoplasm, containing small cells, between them.

As the bodies are cylindrical and more or less closely applied by their

sides, there is more space between them in some places than in others; and it is in these spots, where the bodies cannot come in direct contact, that their intermediate structures are elongated and filiform (Pl. II. figs. 9–14). The filiform arrangement of the granulo-cellular protoplasm is often branched, and a set of elongated masses may unite above or below the bodies. The cells of this intermediate tissue are small and usually spherical; in one kind there is a large refractile nucleus, but in the commonest varieties the cells simply contain granules. It is necessary to study this tissue, because of its close agreement to what I presume to be the nerve-structure, in some, but not in the essential, points. This tissue is clearly continuous with that which has already been noticed as separating and bounding the larger refractile cells outside the Rötteken bodies, and it is continued amongst the small closely set granular cells which underlie these interesting histological elements (Pl. II. fig. 13).

The intermediate tissue binds together the bacilli; for it is continued upwards and between them, the large refractile cells (which I propose to term "Haimean bodies"), and the "Rötteken bodies," and it becomes lost in the cells upon which the proximal ends of these last rest.

It contains the granular structures which give, in the mass, the colour to the chromatophore, and it is evident that the Haimean bodies are developed from it.

The proximal ends of the Rötteken bodies retain their sharp and rounded contour amidst the dense layers of small granular cells which everywhere underlie them.

Those granular cells form a tissue through which light passes with difficulty under the microscope. They are regularly placed in series near the Rötteken bodies; but deeper they become less so, and then other anatomical elements may be observed between them and the muscular fibres upon which the whole chromatophore rests, and which in their turn limit externally the endothelium.

III. *A Notice of Rötteken's discovery of Fusiform Cells and of the different appearances of the Nervous Elements now first observed in the "Plexiform Tissue."*

Rötteken describes these nervous elements as extremely fine fibres and spindle-shaped cells, and asserts that they are probably nerve-fibres and cells. But he has not traced them in conjunction, nor have the fibres been seen of sufficient length to anastomose.

I have found the fusiform bodies and their long ends—the fine fibres mentioned above. Moreover the connexion of these irregular-shaped cells has been determined in these investigations, and the anastomosis of their processes and their connexion with parts of a plexiform nervous tissue also.

These structures are in the midst of a mass of viscous protoplasm,

granules, and granular cells, which merge gradually into the close layers of granular cells under the Rötteken bodies, and they transgress here and there on those layers.

The fusiform cells are numerous (Pl. II. figs. 18–24), and may be divided into two kinds :—(*α*) Those with irregular shapes and short terminal processes, which are prolongations of the cell-wall and are rounded off. These cells contain either highly refractile nuclei, or several nuclei with granular nucleoli. The fusiform shape is not invariable, and in Plate II. fig. 20 a large cell twice the diameter of a Rötteken body is seen amidst the granular plasm. It has a tail-shaped prolongation and some highly refractile nuclei.

β. Those which are rounder in outline, and whose projections are long and continuous with those of others. The outlines of these cells are soft, and without definite and sharp margins, and the colour is a very pale blue-grey. They contain one or more very distinct nuclei. Our type, illustrated in Plate II. fig. 21, has its cells rather wider than a Rötteken body, and they are connected by a process with sharply defined wells—the cell, with many nuclei, having a long caudal fibril of a pale grey colour and rather sharp marginal lines which had suffered disruption.

A second type has large spherical or elliptical cells, which do not have processes passing out in opposite directions, but they are restricted to one part. Usually the cells have only one process, but sometimes two exist close together (fig. 22).

These cells are granular within and have very indistinct nuclei; the cell-wall is extremely delicate, and the whole is of a pale grey colour. The fibrils of these cells are particularly connected with the plexiform tissue. In Plate II. fig. 22 there is a cell with two fibrils—one is short, for it dips down and is foreshortened, and the other is very long; it bifurcates, and one end joins a rounded mass of the plexus, and the other the rugged fibrillar part.

In Plate II. fig. 24 a cell with one fibril is shown. The fibril swells slightly, and then passes down to join a transverse fibre belonging to the plexus.

The plexiform tissue is probably continuous around the *Actinia* beneath the chromatophores, for it is found between the circular band of muscular fibres and every chromatophore. It consists of an irregular main structure and of lateral prolongations, which either anastomose with the fibrils from the fusiform and more spherical cells, or are directly continuous with the cells (fig. 23).

The main structure resembles, in its indistinctness of outline and its pale grey colour and indefinite marginal arrangement, the fibre of the sympathetic of mammals, but it is less coherent and smaller. The usual appearance (Plate II. fig. 23) is that of a grey film with definite branches, and the whole has few granules here and there and a very few

nuclei. It is intimately associated with the surrounding cell-structures, but they may be separated by accident or compression. Here and there the structure enlarges and a ganglion-like cell is seen (Plate II. fig. 22).

I have traced this structure almost across the whole field of the microscope in some sections.

It appears that this portion of the nervous system of *Actinia* (namely, the fusiform and spherical cells with fibrils and the plexiform structure) is distinct histologically from the fibrillar and cellular structures amidst the Haimean and Rötteken bodies. These structures are connective and developing; but it must be remembered that it is possible for both series to come in contact in the midst of the layers of granular cells which underlie the Rötteken bodies.

IV. *Examination into the Physiological Relation between the Chromatophores, the Nerves, and Light.*

The question arises, Are these nerves of special sense? MM. Schneider and Rötteken answer that the small portion of the nervous arrangement they described, *i. e.* the fusiform bodies and their fibrils, are optic nerves. They are satisfied with the physical arrangement of the bacilli, Haimean and Rötteken bodies, and the nature of the colouring-matter imitating that of an organ of vision.

The discovery of the anastomosing fibrils and the plexiform arrangement favour this theory; but there are reasons to be considered which throw much doubt on the views of the distinguished investigators. All *Actinia* have not chromatophores, and closely allied genera may or may not have them. Thus, amongst the *Actinia* with smooth tentacles, there is a group with non-retractile and another with retractile tentacles: amongst those with non-retractile arms are the genera *Anemonia* and *Eumenides* without chromatophores, and *Comactis* and *Ceratactis* with them; amongst the *Actinia* with retractile tentacles are *Actinia* with, and *Paractis* without, chromatophores.

Amongst the tubercular division, the genus *Phymactis* has chromatophores, but its close ally *Cereus* has them not.

Whatever may be the value of this classification of the *Actinia*, it is quite evident that to group together those with and without chromatophores in separate divisions would be the reverse of producing a natural arrangement. It is therefore difficult to believe that these ornaments, with something resembling an optical arrangement, can be the seat of special sensation.

MM. Rötteken and Schneider have observed the large refractile Haimean bodies in the tentacles, and, as will be noticed further on, I have found them of enormous size in the peristome.

They are surrounded in these places, but not covered, with pigment-cells and granules, and are situated just beneath the nematocyst layer in

the tentacle, and beneath a corresponding layer, or one of bacilli, in the peristome. I have failed to recognize any nervous elements in the tentacles save the fusiform bodies, and there are none in the peristome except these irregular cells.

Again, the Haimean bodies are found in the chromatophores, in some places, amidst the Rötteken bodies, separating them.

Nevertheless it is true that light falling on the surface of an *Actinia* will reach further into its structures where there are Haimean bodies, and further still if the Rötteken cells underlie them. Where there is no pigment intervening between the bodies when placed side by side, or between the Rötteken cells, a diffused glare of light would impinge on the granulo-cellular layer below them, in which the nerves ramify and the nerve-cells exist. But when the pigment-granules and cells exist, they break up the general illumination and confine it to a series of separate bright rays. Each of them is brighter than the corresponding space of diffused light; and it would appear that the bacilli, the Haimean bodies, and the Rötteken cells in combination, concentrate light.

Two or three bacilli are placed side by side and behind each other over a small Haimean refractile spherical cell, and perhaps twenty or more cover a large cell (Pl. II. fig. 15). Usually a Haimean body is placed immediately over a Rötteken body; but, as Rötteken has pointed out, this is not an invariable arrangement, for some cover the spaces between and over them. The refractibility of the fluid contents of the Haimean bodies and Rötteken cells appears to be the same; but the elongated form of the last-mentioned structures may act upon light as if their internal fluid were more viscid.

In every instance there is a more or less opaque tissue between the proximal end of the Rötteken body and the nerve-cells; and, moreover, the delicate protoplasmic layer, which is slightly impervious to light, surrounds the Haimean bodies.

In my opinion the Haimean bodies, wherever they exist, carry light more deeply into the tissues than the ordinary epithelial structures. This is also the case with the bacilli and Rötteken bodies, even when they exist separately and with or without the Haimean bodies. There are three ordinary constituents of the skin, and through their individual gifts and structural peculiarity they place the *Actinia* in relation with light. When they are brought together in this primitive form of eye, they concentrate and convey light with greater power, so as to enable it to act more generally on the nervous system—probably not to enable the distinction of objects, but to cause the light to stimulate a rudimentary nervous system to act in a reflex manner on the muscular system, which is highly developed. The *Actinia*, therefore, may feel the light by means of the transparent histological elements when they are separate and constitute integral portions of the ectoderm; but this sensation will be in-

tensified when the three kinds of cells are placed in such order as has been observed in the chromatophores.

The evolution of an eye, which can distinguish outlines, shadows, and colours, probably took the path which is thus faintly indicated in the *Actinia*, which doubtless has an appreciation of the difference between light and darkness.

V. *On the Nerves of the base of* Actinia mesembryanthemum.

A large specimen of a pale green variety from the Mediterranean was examined.

The base being free and expanded, a rapid incision cut out a triangular piece comprehending the ectothelium, the muscular layers, and the mucous endothelium. The apex of the triangle reached the centre of the base of the *Actinia*, and the base of the triangle, which was covered, corresponded with the basal margin of the animal.

Sections were made parallel with the original aspect of the base of the *Actinia*, and then some others at right angles.

The histological elements were studied separately and compared, so that the following tissues could be distinguished readily:—

1. A fibrous-looking tissue like ordinary white fibrous tissue with dark nuclei, to which the muscular fibres are attached and from which they originate.

2. A dense layer of muscular fibres, or rather fibrils, which originates at right angles to the fibre of the fibrous tissue. Each fibril is refractile aud nucleated. Each is separate from its neighbours, and lies in the midst of granules and small cells which contain granules, all being highly refractile. In some places the fibrils are gathered together in masses, so as to leave areolæ between them.

3. Large muscular fibres in contact laterally, so as to form a thin layer. Each fibre is long, broad, has several pale elongate nuclei and a distinct lateral dark line. There are no striæ.

4. The elements of the endothelium and ectothelium, which, as they do not bear on the immediate subject, will be described in a future memoir.

The object of the investigation being to discover some trace of a nervous system, which was presupposed to resemble somewhat the traces observed below the chromatophores, the necessity of becoming familiar with the fibrous and muscular tissues, so as to decide what was not muscle and fibre, is apparent.

I have not found any isolated fusiform cells amongst the tissues of the base; but under the endothelium, and also between the layers of muscular fibres, there are structures which I feel disposed to believe must belong to the nervous system. 1. They are in the position of nerves. 2. Their structure is not that of muscle or fibre. 3. Their structure resembles, in some instances, the plexiform tissues beneath the chromatophores.

The nervous structures are found to present three characteristic shapes:—

1. A thin layer of muscular fibrils of the small and separate (see 2 above) kind, with well-defined dark nuclei in them, was examined. The whole was very transparent and well defined under the $\frac{1}{16}$-inch objective.

Underlying this layer, and extending on either side beyond it, so as to appear in one of the meshes between groups of these fibrils, was a ramified pale grey tissue, which was less pervious to light than the muscular fibrils (Pl. III. fig. 25). Swollen in one part and faintly granular throughout, it had its margins very faintly visible. It was flat, and had a definite resemblance to the widest portion of the plexus already mentioned.

2. A large section of muscular tissue was examined. It consisted of one layer of large muscular fibres (see 3 above) in close lateral contact. Running obliquely over the layer was an irregular but continuous cord ramifying here and there, the branches breaking up into fibrils. In one part the cord was swollen (Pl. III. figs. 26 & 27). A second ramification passed from the opposite end of the field of the microscope and broke up into ultimate fibrils, and in this structure there was a fusiform cell.

Careful manipulation separated a portion of the upper cord from the muscular fibres, but a part of it evidently dropped down amongst them.

3. A layer of muscular fibres of the same kind as those just mentioned was examined. It was marked, as usual, with the lateral dark lines and pale elongated nuclei.

Three long and irregular fibres passed more or less obliquely over the muscular tissue (Pl. III. figs. 28–30). They had distinct lateral or marginal lines, were swollen out in several places, and their texture was faintly granular.

I believe that these fibres were continuous with the fine ramifications of the plexiform arrangement just described.

4. Above the muscular layers, and under the folds of the endothelium, I found an inosculating series of ramifications arising from a common cord. It was situated upon the layer of muscular tissue, with small and separate long fibrils.

The structure was faintly granular, pale grey in colour, with faint outlines, and was swollen in some places: it covered a considerable portion of the field of the microscope; and portions of it had a close resemblance to the ramifying structure mentioned as having been observed below the muscular layer (Pl. III. fig. 31).

The multiplication, if it be justifiable, of these structural elements in the other segments of the base which were not examined would give a fair notion of the plexiform arrangement of the basal nervous tissue. I presume that it consists of a reticulate structure beneath the endothelium, which sends large branches between the vacuities of the most delicate

muscular layer, and which communicates with a ramifying tissue in contact with the other muscular layers, and that this ends in long fibres which supply the wide fibres of this last-mentioned layer.

The diffused nature of this nervous tissue is what might be anticipated would be found in animals possessing such general irritability of tissue, and probably its function is to assist in the reflex movements of the animal, and to produce expansion of the disk on the stimulus of light.

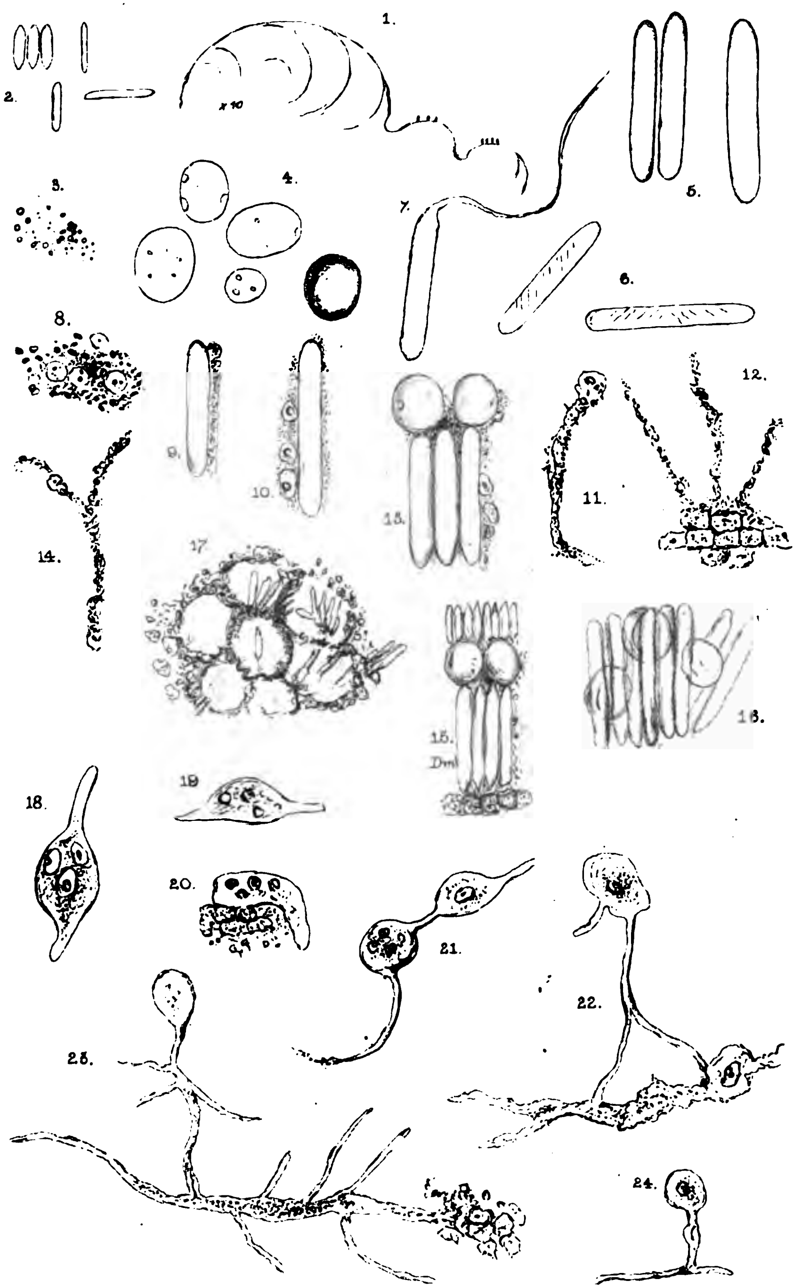
DESCRIPTION OF THE PLATES.

Plate II.

Fig. 1, which is an outline of a chromatophore, with two small ones close to it, is magnified 10 diameters; all the rest are drawn from nature under the magnifying-power of a $\frac{1}{16}$-inch immersion lens and a medium eyepiece.
Fig. 2. Bacilli.
Fig. 3. Granular and cellular protoplasm between bacilli.
Fig. 4. Large refractile cells. Haimean bodies.
Fig. 5. Type α of a Rötteken body.
Fig. 6. „ β „ „ „
Fig. 7. „ γ „ „ „ with a thread.
Fig. 8. Granular and cellular tissue between the Haimean bodies.
Fig. 9. Same kind of tissue in contact with a Rötteken body.
Fig. 10. Some cells with refractile nuclei in the tissue.
Fig. 11. Portion of tissue from amongst the Rötteken bodies.
Fig. 14. The same, with a forked end.
Fig. 12. Three portions of intermediate tissue ending in the layer of granular cells which underlies the Rötteken bodies.
Fig. 13. Haimean and Rötteken bodies and the intermediate tissue in position.
Fig. 15. A diagram, but very close to nature, of the relative position of the histological elements of the chromatophores.
Fig. 16. Haimean and Rötteken bodies intermingled.
Fig. 17. Haimean bodies surrounded by pigment-cells, and with bacilli flat upon them, owing to pressure.
Figs. 18 & 19. Fusiform nerve-cells.
Fig. 20. A nerve-cell.
Fig. 21. Nerve-cells connected and with fibres.
Fig. 22. A spherical nerve-cell with processes joining the plexus.
Fig. 23. Ramifications of the plexiform cord.
Fig. 24. Nerve-cell and fibres.

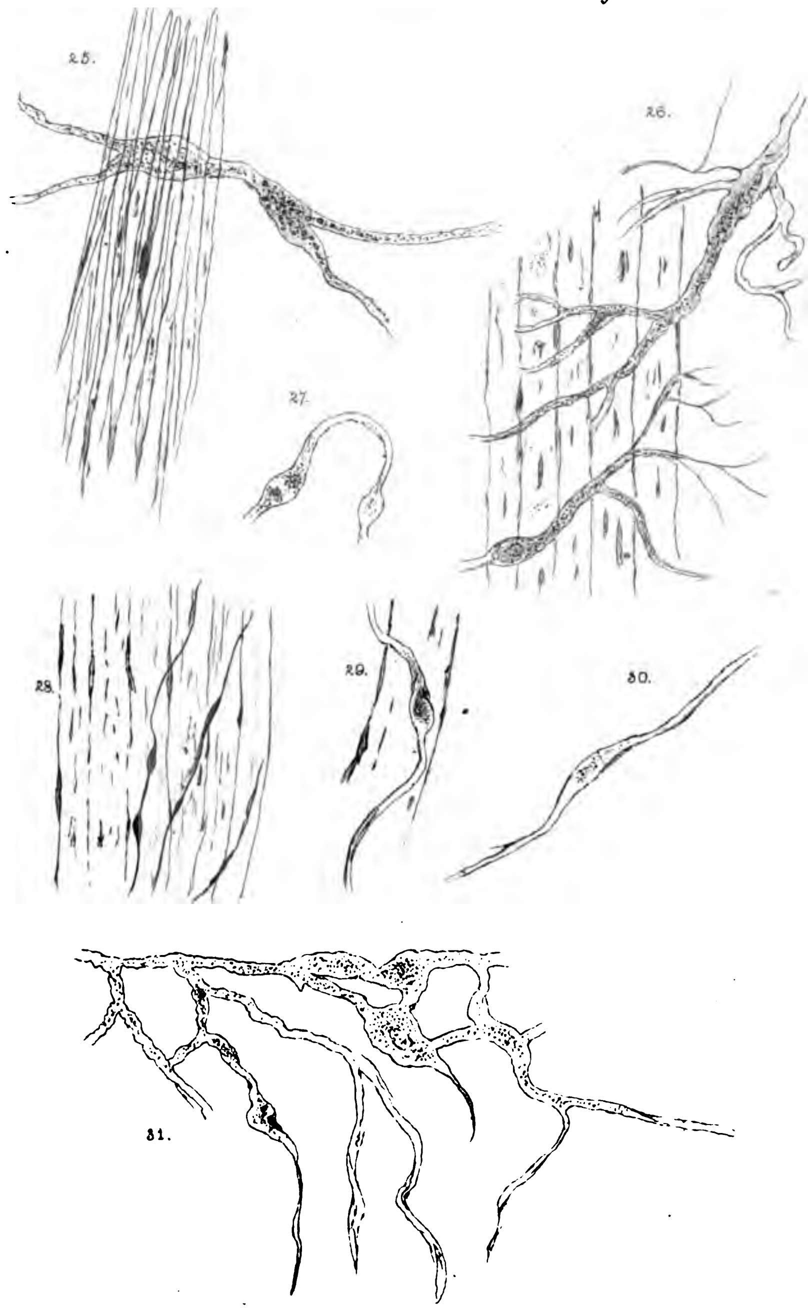
Plate III.

Fig. 25. Nerve in relation to the small muscular fibrils of the base.
Fig. 26. Nerve ramifying and supplying wide muscular fibre.
Fig. 27. A loop of nervous fibre.
Fig. 28. Terminal ends of the plexus passing over muscular fibre.
Figs. 29 & 30. The same, more highly magnified.
Fig. 31. The plexus under the endothelium.



P. M. D. ad. nat. del.
W. H. Wesley lith.

W. West & C° imp.



P.M.D. ad. nat del.
W.H. Wesley lith.

W. West & C^{o} imp.

April 16, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Pneumatic Action which accompanies the Articulation of Sounds by the Human Voice, as exhibited by a Recording Instrument." By W. H. Barlow, F.R.S., V.P.Inst.C.E. Received February 23, 1874.

All articulated sounds made by the human voice are accompanied by the expulsion of air from the mouth; and in a series of articulated sounds the air is ejected in impulses which vary in quantity and pressure, and in the degree of suddenness with which they commence and terminate.

It appeared to me that it would be interesting and probably useful, as tending to elucidate the process and effects of articulation, to construct an instrument which should record these pneumatic actions by diagrams, in a manner analogous to that in which the indicator-diagram of a steam-engine records the action of the engine.

In considering a suitable form of recording instrument, the conditions to be met were:—first, that the pressures and quantities were very variable, some of them being extremely small; and, secondly, that the impulses and changes of pressure follow each other occasionally with great rapidity.

It was therefore necessary that the moving parts should be very light, and that the movement and marking should be accomplished with as little friction as possible.

The instrument I have constructed consists of a small speaking-trumpet about 4 inches long, having an ordinary mouthpiece connected to a tube $\frac{1}{2}$ an inch in diameter, the other end of which is widened out so as to form an aperture of $2\frac{1}{4}$ inches diameter.

This aperture is covered with a membrane of goldbeater's skin or thin gutta percha.

A spring which carries the marker is made to press against the membrane with a slight initial pressure, to prevent as far as practicable the effects of jar and consequent vibratory action.

A very light arm of aluminium is connected with the spring and holds the marker; and a continuous strip of paper is made to pass under the marker in the same manner as that employed in telegraphy.

The marker consists of a small fine sable brush placed in a light tube of glass $\frac{1}{10}$ of an inch in diameter. The tube is rounded at the lower end, and pierced with a hole about $\frac{1}{20}$ of an inch in diameter. Through this hole the tip of the brush is made to project, and it is fed by colour

appears to depend on the force required in the last syllable, if more than one are consecutively uttered, and is most developed in those syllables terminating with the consonants termed "Explodents," whether with or without the silent vowel E after them.

This effect is exhibited in fig. 1.

Fig. 1.

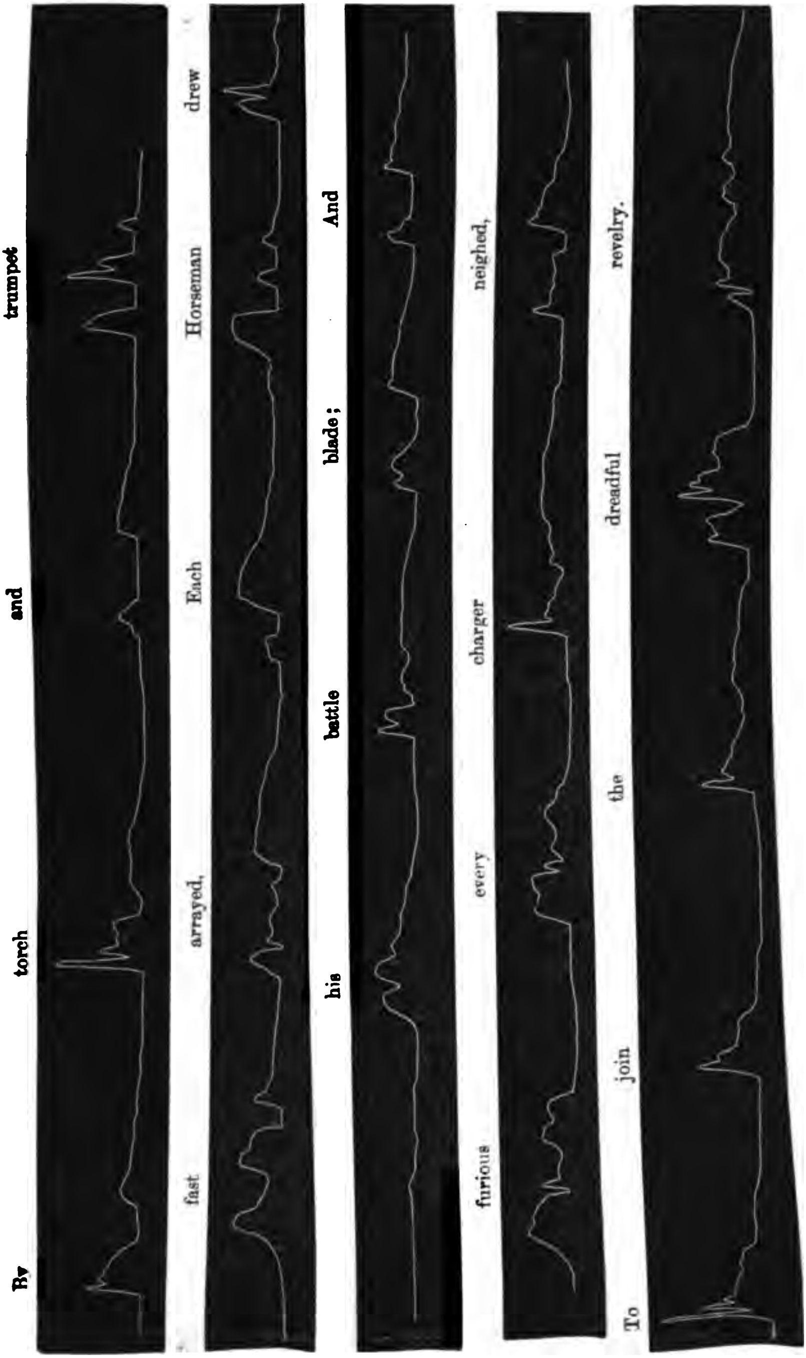
Let. Attempt. Integrate.

Fig. 2.

Let. Attempt. Integrate.

Fig. 3.

Letter. Attempting. Integrating.


By torch and trumpet fast arrayed,
Each Horseman drew his battle blade;
And furious every charger neighed,
To join the dreadful revelry.

It will be observed that the diagrams of the separate words, although they become modified when grouped together, are more or less discernible in the lines continuously spoken; and the similarity of sound at the termination of the first three lines, which constitutes the rhyme of the verse, is represented in the similarity of form, or in the character of the form, of the terminations of the diagrams of these three lines.

The subject might be pursued much further by showing the diagrams of the same words spoken by different individuals, the outlines produced by the words and sentences of other languages, the effect produced by change of accent, &c.

My object, however, has not been to pursue the subject into minute detail, but to show that the articulation of the human voice is accompanied by definite pneumatic actions, and that those actions, many of which are insensible to ordinary observation, are capable of being recorded.

II. "Note on the Periodicity of Rainfall." By J. H. N. Hennessey, Esq., F.R.A.S. Communicated by Prof. G. G. Stokes, Sec.R.S. Received February 24, 1874.

1. Interested in the inquiry proposed by Mr. Meldrum, as to whether rainfall varies with the sun-spot area, I examined the register kept at the office of the Superintendent of the Great Trigonometrical Survey of India, and am enabled, through the courtesy of Colonel J. T. Walker, R.E., to communicate the results. These are probably not devoid of peculiar interest, from the abnormal conditions presented by the stations of observation, which are far inland, and on, or adjoining, lofty mountains, as appears from the following brief descriptions.

2. Mussoorie station is on the southernmost range of the Himalaya Mountains, lat. N. 30° 28′, long. E. 78° 7′, height 6500 feet; this range rises suddenly and forms the northern boundary of the Dehra *Doon* (or Dehra valley), which is some 18 miles wide and 40 miles long, and is bounded to the south by the Sewalik range of hills, about 3500 feet high. Dehra station is 2200 feet high, 10 miles south of Mussoorie station, and in the Dehra valley.

3. Owing to the absence of the observers in the winter months from Mussoorie station, the rainfall is not recorded there during that period; this, however, is of little consequence to the inquiry in hand, for the total annual fall occurs almost entirely in June, July, and August. I accordingly give in Table I. the total fall at Mussoorie between May 1 and October 31 of each year; and in order to make these totals comparable at the two stations, if desired, the fall for January, February, March, April, November, and December is excluded from the Dehra totals; this quantity excluded may be set down at some 6 inches, or only

some 7½ per cent. of the *annual* fall. Excepting five years at Dehra and two at Mussoorie, all the observations have been taken under my own superintendence, so that I can vouch for their accuracy. Rejecting decimal places as redundant, the rainfall is as follows (in inches) for 20 years at Mussoorie and for 13 years at Dehra :—

TABLE I.

Sun-spot area*.	Year (May 1 to Oct. 31).	Rainfall, in inches, at	
		Mussoorie station.	Dehra station.
	1854	101	
	1855	86	
Minimum	1856	93	
	1857	88	
	1858	85	
	1859	78	
Maximum	1860	66	
	1861	141	103
	1862	94†	110
	1863	93†	77†
	1864	82	72
	1865	76	67
	1866	81	75†
Minimum	1867	82	70
	1868	61	45
	1869	52	65
	1870	80	84
Maximum (?)	1871	84	114
	1872	83	83
	1873	82	63

4. Adding to the fall in the epochal year (*i. e.* maximum or minimum) the fall for one preceding and one succeeding year, we shall get what may be termed *three-year sums*; similarly, by including two years on each side of the epochal year, we find *five-year sums*‡: the results are as follows :—

TABLE II.

	3-year sums, in inches.		5-year sums, in inches.	
	Mussoorie.	Dehra.	Mussoorie.	Dehra.
1856. Minimum................	267		453	
1860. Maximum	285		464	
1867. Minimum................	224	190	352	322
1871. Maximum	247	281	381	409

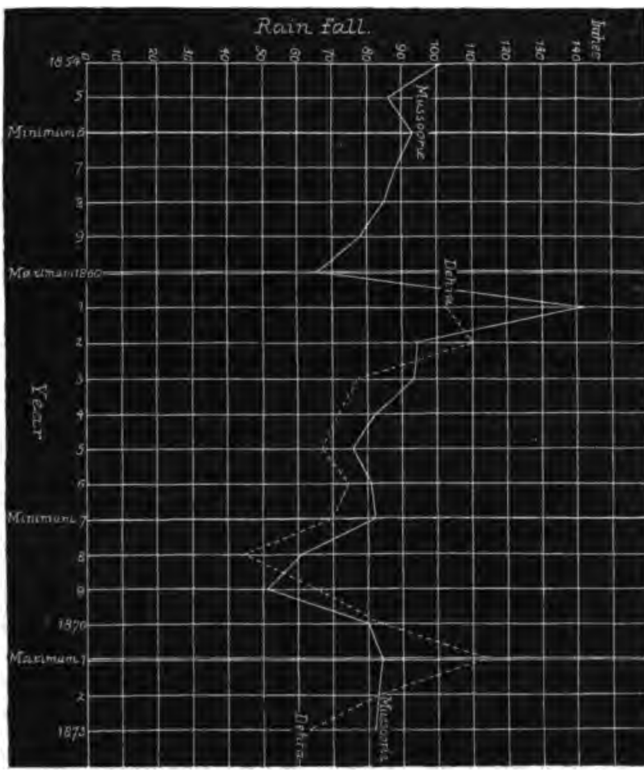
* Taken from a paper in 'Nature,' 1872, December 12, page 100, by Norman Lockyer, Esq., F.R.S., &c.

† Site of rain-gauge shifted.

‡ 'Nature,' 1872, December 12.

Notwithstanding the exceptional localities of the stations, the above results are generally in keeping with the Meldrum theory: the Dehra observations for 1860 and prior years are unfortunately wanting; but it will be seen in Table I. that heavy falls occurred in the two years succeeding the epochal year 1860.

5. It may, however, be questioned whether stations inland *are* ineligible to test the theory under notice. No doubt far more rain falls on certain parts of the globe than on others, and Mussoorie and Dehra are included in the former: but a large rainfall is in fact a recommendation, presenting as it does a large measure of the periodicity in question; so that stations under this condition appear highly eligible unless the rainfall is subject to abnormal fluctuations, apart from the supposed influence of sun-spot area; indeed, were it practicable to measure the total rainfall on the whole globe, the total results would present the most effective argument for periodicity. Projecting the facts of Table I., with the help of ordinates and abscissæ we obtain the appended diagram, where I am unable to



introduce, in lieu of the year, numerical values of sun-spot areas from want of complete results, such as those obtained by Messrs. De La Rue,

Balfour Stewart, and Loewy. Recognizing the sun as the governor of our system and the source of terrestrial heat and light, it appears certain that at least some of the circumstances attending our globe are directly or indirectly the results of solar conditions, of which we can read but too few, and interpret still fewer rightly. In the present instance we see that, as in other curves, a certain rainfall maximum may be *less* than minima not immediately preceding or succeeding; and this alone suggests the desirability of comparison with actual magnitudes of sun-spot areas; but the introduction of this more accurate test would doubtless prove a waste of time, unless the approximate relation at present under view can be maintained.

III. "Studies on Biogenesis." By William Roberts, M.D., Manchester. Communicated by Henry E. Roscoe, F.R.S. Received March 3, 1874.

(Abstract.)

The object of the investigation is to inquire into the mode of origin of *Bacteria* and toruloid vegetations. The inquiry is divided into three sections.

Section I. *On the sterilization by heat of organic liquids and mixtures.*—When beef-tea or a decoction of turnip is boiled for a few minutes and afterwards preserved from extraneous contamination, it passes into a state of "permanent sterility."

This state is characterized by loss of power to *originate* organisms with conservation of the power of *nourishing and promoting the growth* of organisms.

All organic liquids and mixtures seem capable of being brought to this state by exposure to the heat of 212° F.; but the length of time during which exposure to this heat is necessary to induce sterilization varies greatly according to the nature of the materials. Ordinary infusions and decoctions were sterilized by boiling for five or ten minutes; but milk, chopped green vegetables in water, pieces of boiled egg in water, and other mixtures were not sterilized unless the heat was continued for twenty to forty minutes. Hay-infusion was sterilized, like other infusions, by boiling for a few minutes; but when the infusion was rendered alkaline with ammonia or liquor potassæ, it was not sterilized except after an exposure to the heat of boiling water for more than an hour. Sometimes it germinated after two hours, and once after three hours of such exposure.

There appeared to be two factors of equal importance in the induction of sterilization—namely, the *degree* of heat and the *duration* of its application. These two factors appeared to be mutually compensatory in such fashion that a longer exposure to a lower temperature was equivalent to

a shorter exposure to a higher temperature. For example, speaking roughly, an exposure for an hour to a heat of 212° F. appeared to be equivalent to an exposure for fifteen minutes to a heat of 228° F.

SECTION II. *On the capability of the normal tissues and juices to generate* Bacteria *and* Torulæ *without extraneous infection.*—The following substances were examined :—egg-albumen, blood, urine, blister-serum, milk, grape, orange- and tomato-juice, turnip and potato. These substances were conveyed into previously prepared sterilized bulbs and tubes, which were hermetically sealed at one end and plugged with cotton-wool at the other end. When the several steps of the experiment were quickly and dexterously performed, the risks of extraneous contamination, although not altogether avoided, were reduced to small proportions. The bulbs and tubes thus charged were afterwards maintained at a temperature ranging from 60° to 90° F., and were finally examined at periods varying from four to ten weeks. Out of 90 experiments performed in this way, 67 preparations remained barren and 23 became fertile. When the ideal conditions of the experiment could be carried out in approximative perfection, as with urine, blister-serum, orange-, grape-, and tomato-juice (34 experiments), the preparations, all save one, remained barren; but when the risks of extraneous infection were (from the mechanical difficulties) obviously greater, as with blood, milk, turnip, and potato, the proportion of fertile preparations was considerable, though even with these (except in the case of milk) the barren preparations were in a large majority.

The experiments seemed clearly to lead to the conclusion that the normal tissues of plants and animals were incapable of breeding *Bacteria* and *Torulæ* except under the stimulus of extraneous infection.

SECTION III. *On the bearing of the facts adduced in the preceding sections on the origin of* Bacteria *and* Torulæ, *and on the real explanation of some of the alleged cases of Abiogenesis.*—Seeing that organic liquids and mixtures sterilized by heat, and the normal juices and tissues, continued permanently barren under the most favourable conditions of air, moisture, warmth, and light, so long as they were preserved from extraneous contamination, and seeing that the admission of ordinary air or water into contact with them was invariably followed by germination, it was impossible to avoid the conclusion that ordinary air and water contain, in addition to their proper elements, multitudes of particles capable of provoking germination. The exact nature of these particles may be a matter of dispute, but the reality of their existence is not doubtful; nor is it doubtful that the ordinary and common development of *Bacteria* and *Torulæ* is directly due to their agency.

The greatest difficulty hitherto encountered to the general acceptance of the panspermic theory has been the appearance of *Bacteria* (without the possibility of fresh infection) in certain liquids which have been exposed for a considerable time to a boiling heat. Only two explanations

of this fact seem possible—either germs preexisting in them have survived the heat, or the organisms have arisen in them abiogenically. These alternatives were subjected to two series of test experiments. In the first series it was proved directly that there exist in ordinary air and water particles which preserve their germinal activity after being boiled for five minutes in previously sterilized liquids. The second series of experiments showed that, in the extraordinary increase of resistance to sterilization by heat exhibited by alkalized hay-infusion, the action of the alkali is to heighten the surviving power of preexisting germs, and not to exalt the abiogenic aptitude of the infusion itself.

The issue of the whole inquiry has been to fully confirm the main propositions of the panspermic theory, and to establish the conclusion that *Bacteria* and *Torulæ*, when they do not proceed from visible parents like themselves, originate from invisible germs floating in the surrounding aërial and aqueous media.

Nevertheless the author is unable to withstand the impression that this general and common mode of origin is possibly supplemented, under rare conditions, by another and an abiogenic mode of origin. The facts on which this impression rests are comparatively few. They consist in certain instances of greatly retarded germination of *Bacteria* in liquids which had been exposed to a boiling heat, and in two very remarkable instances of the growth of fungoid vegetations (not identical with those usually developed after air infection) in plugged bulbs which had been boiled in a can of water.

If it should be hereafter established that *Bacteria* and fungoid vegetations do, under exceptional circumstances, arise abiogenically, this would not overturn the panspermic theory, it would merely limit the universality of its application.

April 23, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On some Points connected with the Circulation of the Blood, arrived at from a study of the Sphygmograph-Trace." By A. H. Garrod, B.A., Fellow of St. John's College, Cambridge; Prosector to the Zoological Society. Communicated by Prof. A. B. Garrod, M.D., F.R.S. Received March 12, 1874.

(Abstract.)

The author commences by giving a table containing a fresh series of

measurements of the ratio borne by the cardiosystole * to its component beat in the cardiograph-trace. These tend strongly to substantiate the law previously published by him, viz. that the length of the cardiosystole is constant for any given pulse-rate, and that it varies as the square root of the length of the pulse-beat only—being found from the equation $xy=20\sqrt{x}$, where x=the pulse-rate, and y=the ratio borne by the cardiosystole to the whole beat.

A similar series of fresh measurements are given in proof of the law previously published by him, that in the sphygmograph-trace from the radial artery at the wrist the length of the sphygmosystole † is constant for any given pulse-rate, but varies as the cube root of the length of the pulse-beat—it being found from the equation $xy'=47\sqrt[3]{x}$, where x= the pulse-rate, and y'=the ratio borne by the sphygmosystole to the whole beat.

By measurement of sphygmograph-tracings from the carotid in the neck and the posterior tibial artery at the ankle, it is then shown that the length of the sphygmosystole in those arteries is exactly *the same* as in the radial; so that the above-stated law as to the length of the sphygmosystole in the latter applies to them also, and must therefore apply equally to the pulse in the aorta.

Such being the case, by comparing the equation for finding the length of the cardiosystole with that for finding the aortic sphygmosystole, the relation between the duration of the whole cardiac systolic act and the time during which the aortic valve remains open can be estimated with facility; for by subtracting the shorter sphygmosystole from the longer cardiosystole, a remainder is obtained which can be nothing else than the expression of the time occupied by the ventricle at the commencement of its systole in elevating its internal pressure to that of the blood in the aorta, which must occur before the aortic valve can open up. This interval is named the "*syspasis*." Its length is found to be constant for any given pulse-rate, but to decrease very rapidly with increase in rapidity of the heart's action, becoming *nil* when that reaches 170 a minute. An attempt is made to explain these phenomena.

If the above considerations are correct, certain independently obtained measurements ought, on comparison, to correspond; for by reference to one of the author's papers in the 'Proceedings of the Royal Society,' it is shown that the length of the there-termed second cardio-arterial interval (which may be called the second cardio-radial interval) can only represent the time taken by the second or dicrotic pulse-wave in travelling from the aortic valve to the wrist. This being so, there is every *à priori* reason in favour of the earlier primary wave taking the same time in going

* The *cardiosystole* is the interval between the commencement of the systole and the closure of the aortic valve in each cardiac revolution.

† The *sphygmosystole* is the interval between the opening and closing of the aortic valve in each cardiac revolution.

the same distance—which can be expressed in other terms by saying that the length of the first cardio-radial interval, from which that of the syspasis has been subtracted, ought to be exactly the same as that of the second cardio-radial interval. That such is the case is proved by the measurement of the lengths of these two intervals independently; and it is found that in all cases they agree *to three places of decimals*, which is great evidence in favour of the accuracy of the methods and arguments employed.

The latter part of the paper is occupied with the description of, and the results obtained by, the employment of a double sphygmograph, by means of which simultaneous tracings are taken from two arteries at different distances from the heart. The arteries experimented on are the radial at the wrist and the posterior tibial just behind the ankle, 29 and 52·5 inches respectively from the aortic valves. From the resulting tracings, the time occupied by the pulse-wave in travelling the difference of distance (52·5 − 29 =), 23·5 inches, is found and stated to be 0·0012 of a minute in a pulse of 75 a minute; and it is shown that this varies very little with differences in pulse-rate, as might have been previously expected; it is also proved that *there is an appreciable acceleration of the movement of the pulse-wave as it gets further from the heart.*

By superposing the simultaneous trace from the wrist on that from the ankle, direct verification is obtained of the earlier proposition—that the sphygmosystole at the wrist and that at the ankle are of exactly similar duration. The peculiarities of the ankle-trace are also referred to.

II. "Note on the Minute Anatomy of the Alimentary Canal." By Herbert Watney, M.A. Cantab. Communicated by Dr. Sanderson, F.R.S., Professor of Practical Physiology, University College. Received March 10, 1874.

The following results relating to the anatomy of the mucous membrane of the alimentary canal were obtained in the laboratory of the Brown Institution. The researches were carried out under the direction of Dr. Klein.

1. *Connective-tissue corpuscles amongst the epithelium.*—In specimens hardened in chromic acid and alcohol and stained in hæmatoxylin, structures are constantly seen among the columnar epithelium of the intestinal tract in many animals (as monkey, sheep, cat, dog, rat, rabbit) which belong to the connective tissue. These are:—(1) a delicate reticulum, which is continuous with that formed by the most superficial layer of connective-tissue corpuscles (the basement membrane); (2) round nucleated cells, exactly similar to those of the mucosa.

This is the case at the pyloric end of the stomach, on the villi, over Peyer's patches, and in Lieberkühn's glands.

2. The lining endothelium of the lymph-vessels of the mucosa is in anatomical continuity with the reticulum of nucleated cells (connective-tissue stroma); so that it may be said the endothelial cells of the lymphatic vessel are only transformed connective-tissue corpuscles.

3. In animals killed during the absorption of fat (cream) the fat can be seen in preparations stained by osmic acid as small black particles:—(1st) arranged in lines between or around the epithelial cells; (2ndly) in the basement membrane; (3rdly) as has been noticed by many previous observers, in the connective-tissue stroma of the villus, whence it can be traced into the lymph-vessel. This indicates that the fat is absorbed by the processes of the connective tissue which exist between the epithelial cells, and thence finds its way by the connective-tissue stroma to the lymph-vessel.

4. The reticulum of nucleated cells of the mucosa forms a special sheath to the vessels and unstriped muscular tissue.

In the villi the muscular bundles, having approached the apex, terminate, the connective tissue which forms their sheath being continuous with the corpuscles forming the basement membrane.

In the mucosa of the colon of the rabbit the slender muscle-bands divide into single muscle-fibres, on which the common sheath is continued. This sheath becomes often connected with peculiar large, oval, nucleated cells lying close under the epithelium.

5. *State of the mucous glands of the tongue in rest and secretion.*—It has been found, in accordance with the researches of Professor von Ebner, of Graz, that there are two kinds of acinous glands in the tongue, which have been distinguished as serous and mucous—the former being always found in relation to the papillæ vallatæ and circumvallatæ, the latter always at the root of the tongue and partially surrounding the former.

In the course of the present inquiry it has been further found (in sections stained in hæmatoxylin and carmine, made from the hardened tongue of an animal which had been left for a few hours without food) that the two kinds of glands are coloured red and blue respectively; but in sections of the tongue of an animal killed while feeding, both kinds of glands were stained red, while any mucus in the duct of the mucous glands was stained blue—showing that, in the state of inanition, the cells of the mucous glands contain mucus, while, during secretion, the cell-substance is affected by the staining fluids in a manner not unlike that in which ordinary cell-substance would be acted on.

III. "On the Refraction of Sound by the Atmosphere." By Prof. OSBORNE REYNOLDS, Owens College, Manchester. Communicated by Prof. STOKES, Sec.R.S. Received March 18, 1874.

(Abstract.)

The principal object of this paper is to show that sound is refracted upwards by the atmosphere in direct proportion to the upward diminution of the temperature, and hence to explain several phenomena of sound, and particularly the results of Prof. Tyndall's recent observations off the South Foreland.

The paper commences by describing the explanation of the effect of wind upon sound, viz. that this effect is due to the lifting of the sound from the ground, and not to its destruction, as is generally supposed.

The lifting of the sound is shown to be due to the different velocities with which the air moves at the ground and at an elevation above it. During a wind the air moves faster above than below, therefore sound moving against the wind moves faster below than above, the effect of which is to refract or turn the sound upwards; so that the "*rays*" of sound, which would otherwise move horizontally along the ground, actually move upwards in circular or more nearly hyperbolic paths, and thus, if there is sufficient distance, pass over the observer's head. This explanation was propounded by Prof. Stokes in 1857, but was discovered independently by the author.

The paper then contains the description of experiments made with a view to establish this explanation, and from which it appears that :—

1. The velocity of wind over grass differs by one half at elevations of 1 and 8 feet, and by somewhat less over snow.

2. When there is no wind, sound proceeding over a rough surface is destroyed at the surface, and is thus less intense below than above.

3. That sounds proceeding against the wind are lifted up off the ground, and hence the range is diminished at low elevations; but that the sound is not destroyed, and may be heard from positions sufficiently elevated with even greater distinctness than at the same distances with the wind.

4. That sounds proceeding with the wind are brought down to the ground in such a manner as to counterbalance the effect of the rough surface (2); and hence, contrary to the experiments of Delaroche, the range at the ground is greater with the wind than at right angles to its direction, or where there is no wind.

On one occasion it was found that the sound could be heard 360 yards with the wind at all elevations, whereas it could be heard only 200 yards at right angles to the wind, standing up; and, against the wind, it was lost at 30 yards at the ground, 70 yards standing up, and at 160 yards at an

elevation of 30 feet, although it could be heard distinctly at this latter point a few feet higher.

As might be expected, the effect of raising the bell was to extend its range to windward, to even a greater extent than was obtained by an equal elevation of the observer.

These results agree so well with what might be expected from the theory as to place its truth and completeness beyond question.

It is thus argued that, since the wind raises the sound so that it cannot be heard at the ground, by causing it to move faster below than above, any other cause which produces such a difference in velocity will lift the sound in the same way; and therefore that an upward diminution in the temperature of the air must produce this effect; for every degree of temperature between 32° and 70° adds nearly one foot per second to the velocity of sound. Mr. Glaisher's balloon observations * show that when the sun is shining with a clear sky, the variation from the surface is 1° for every hundred feet, and that with a cloudy sky 0°·5, or half what it is with a clear sky. Hence it is shown that "rays" of sound, otherwise horizontal, will be refracted upwards in the form of circles, the radii of which are 110,000 feet with a clear sky, and 220,000 with a cloudy sky —that is to say, the refraction on bright hot days will be double what it is on dull days, and still more under exceptional circumstances, and comparing day with night.

It is then shown by calculation that the greatest refraction (110,000 radius) is sufficient to render sound, from a cliff 235 feet high, inaudible on the deck of a ship at 1¾ mile, except such sound as might reach the observer by divergence from the waves passing over his head; whereas, when the refraction is least (220,000 radius), that is, when the sky is cloudy, the range would be extended to 2½ miles, with a similar extension for the diverging waves, and under exceptional circumstances the extension would be much greater. It is hence inferred that the phenomenon which Prof. Tyndall observed on the 3rd of July and other days (namely, that when the air was still and the sun was hot he could not hear guns and other sounds from the cliffs 235 feet high more than 2 miles, whereas when the sky clouded the range of the sounds was extended to 3 miles, and, as evening approached, much further) was due, not to the stoppage or reflection of the sound by clouds of invisible vapour, as Prof. Tyndall has supposed, but to the sounds being lifted over his head by refraction in the manner described; and that, had he been able to ascend 30 feet up the mast, he might at any time have extended the range of the sounds by a quarter of a mile at least.

* Brit. Assoc. Report, 1862, p. 462.

April 30, 1874.

Prof. ANDREW CROMBIE RAMSAY, LL.D., Vice-President, in the Chair.

It was announced from the Chair that the President and Council had appointed Mr. Lockyer's Paper, "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun, No. III.," read Nov. 27 last, to be the Bakerian Lecture; and Dr. Ferrier's Paper, on "the Localization of Function in the Brain," read March 5 last, to be the Croonian Lecture for the present year.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Structure of the Mucous Membrane of the Uterus and its Periodical Changes." By JOHN WILLIAMS, M.D. (Lond.), Assistant Obstetric Physician to University College Hospital. Communicated by Dr. SHARPEY. Received March 21, 1874.

(Abstract.)

The paper consists of observations made on the uteri of nine women who had died in different stages of the monthly period.

In two of the uteri the menstrual flow had almost ceased, and the mucous membrane was wanting in the bodies of the organs. The muscular fibre-cells were more or less exposed in the cavity, and the meshes formed by their bundles contained glands and groups of round cells.

In one uterus menstruation had ceased three days before death, and the muscular fibres were not exposed in the cavity of the organ, but imposed upon them was a layer of tissue composed of fusiform and round cells. This tissue contained glands. The muscular tissue near the internal orifice was devoid of glands, but nearer the fundus it contained numerous glands.

In one uterus, in which the catamenial flow had ceased probably about a fortnight before death, the layer of superficial tissue was thicker than in the last; and near the internal orifice there was a marked and abrupt distinction between it and the subjacent muscular tissue.

In one uterus the flow had ceased three weeks before death, and the superficial layer was still thicker; and the distinction between it and the subjacent muscular layer was well marked, except at the fundus. The uterine glands were tubular, and arranged in some parts obliquely, in others perpendicularly to the surface. They were lined by columnar ciliated epithelium.

In two uteri menstruation was imminent, but the flow had not begun. In these the mucous membrane of the body of the uterus was fully developed, and had begun to undergo fatty degeneration. There was a marked distinction between it and the muscular tissue throughout the uterine cavity: it was highly congested.

In one uterus the menstrual flow had taken place for one day, and in another for two or three days before death. In these there was extravasation of blood into the mucous membrane, and the latter had in part been disintegrated and removed.

Menstruation appears essentially to consist, not in a congestion or a species of erection, but in growth and rapid decay of the mucous membrane. The menstrual discharge consists chiefly of blood and of the débris of the mucous membrane of the body of the uterus. The source of the hæmorrhage is the vessels of the body of the uterus. The mucous membrane having undergone fatty degeneration, blood becomes extravasated into its substance; then the membrane undergoes rapid disintegration, and is entirely carried away with the menstrual discharge. A new mucous membrane is then developed by proliferation of the inner layer of the uterine wall, the muscular tissue producing fusiform cells, and the groups of round cells enclosed in the meshes of the muscular bundles producing the columnar epithelium of the glands.

II. "On Leaf-Arrangement." By Hubert Airy, M.A., M.D. Communicated by Charles Darwin, F.R.S. Received March 23, 1874.

(Abstract.)

This paper is offered in correction and extension of the views contained in a previous paper by the same author, read 27th February, 1873.

The main facts of leaf-arrangement to be accounted for are:—

(1) the division into *verticillate* and *alternate* leaf-order;

(2) in the former, the equal division of the circumference of the stem by the leaves of each whorl, and the alternation, in angular position, of successive whorls;

(3) in the latter, the arrangement of leaves in a spiral series round the stem, with uniform angular divergence between successive leaves, and the limitation of that angular divergence (represented as a fraction of the circumference) to certain fractional values (in most cases only approximate) which find place most commonly in the following convergent series (A):—

$$\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}, \frac{8}{21}, \frac{13}{34}, \frac{21}{55}, \frac{34}{89}, \frac{55}{144}, \text{\&c.}; \ldots\ldots\ldots\ldots (A)$$

more rarely in the following (B):—

$\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}, \frac{8}{29}, \frac{13}{47}$, &c.; ....(B)

very rarely in the following (C):—

$\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c.;...........................(C)

besides a few isolated values, $\frac{2}{11}, \frac{2}{13}, \frac{1}{8}$, &c., which would find place in higher series. (Hofmeister, 'Allgemeine Morphologie der Gewächse,' p. 449. Leipzig, 1868.)

Dealing first with the phenomena of *alternate* leaf-order, the theory is advanced that, in each of the series A, B, C, &c., the higher orders have been derived from some lower order of the same series by a process of condensation advantageous to the species in which those higher orders are found; that the scene of this condensation of leaf-order has been the bud and other close-packed forms of plant-growth; and that the immediate gain has been better economy of space.

In support of this theory it is argued, first, that the *use* of leaf-order is to be found in that stage of the life of a shoot in which the leaf-order is most regular and perfect. Leaf-order is seen in perfection in close-packed forms of plant-growth, such as the *bud*, the *bulb*, the *radical rosette*, the *involucre*, the *composite head*, the *catkin*, the *cone*, even the *seed* itself. Therefore it must be in these forms that leaf-order is especially useful. In elongated shoots, on the contrary, with long internodes and distant leaves, the leaf-order has a tendency to lose that regularity which it enjoyed in the bud, and is often disarranged by a twist of the stem or by contortion of the leaf-stalks (required for the better display of the leaf-blades to the light). The native arrangement of the leaves (excluding the order $\frac{1}{2}$) is often a positive disadvantage to them in lateral twigs. It is only in the more vertical and unembarrassed shoots that the leaf-blades remain content with their distributive position. Indeed, one chief use of the leaf-stalk seems to be to enable the leaf-blade to make the best of an unfavourable birth-place. (Yew, silver fir, box, and privet are instanced as examples.) Hence it appears probable that the use of leaf-order is not to be found in the elongated shoot.

Looking, then, to the above-mentioned close-packed forms of plant-growth as the scene of the usefulness of leaf-order, it is seen that the characteristic feature which distinguishes them from the elongated forms is *contact* between neighbouring leaves (or shoots). The whole surface of the stem is occupied by their bases, and no vacant interstices are left between them. It is plain that the process of cell-growth has resulted in great *mutual pressure* between neighbouring leaves and shoots. Recognizing

this fact of mutual pressure, we can see that leaf-order is useful in these close-packed forms by securing equal development of leaves and therefore economy of space. If the whole space is to be occupied, and the leaves or shoots are to have equal development, there must be orderly arrangement of some kind. The principle of economy of space under mutual pressure is put forward as of chief importance in leaf-arrangement.

It appears that economy of space is especially demanded in a longitudinal direction, for the sake of protection against vicissitudes of temperature and the attacks of enemies. In a bud, for example, it is evidently important, on the one hand, that as many leaves as possible should attain as high development as their situation will allow, in order that they may be ready at the first approach of spring to complete that development and enter on their function without loss of time; but, on the other hand, it is evidently important that the embryo shoot should be as short as possible, in order that it may be well within the guard of the protecting scales and less exposed to danger during the long period of bud-life. These claims will be satisfied by a vertical condensation of the leaf-order, such as the state of mutual pressure of the embryo leaves and shoots is calculated to bring about.

That the arrangements represented by the lower terms of the above-mentioned series A, B, C, &c. would, under a force of longitudinal condensation, actually give rise to the successive arrangements represented by the higher terms of the same series, is shown by diagrams, in which the necessary consequences of each step of condensation are made apparent to the eye. In these diagrams a leaf or shoot is represented (for mechanical considerations) by a sphere, and the spheres are numbered from 0 upwards. Taking, first, series A, the lowest order of that series, $\frac{1}{2}$, is represented by two vertical rows of spheres, those of each row being in contact and alternating with those of the other. If these two rows remain vertical, no longitudinal condensation can take place. The first step towards such condensation must be their spontaneous deviation from the vertical. (Instances of such deviation in nature are found in the genus *Gasteria* and others, to be considered further on.) The next step required is some force of vertical compression, such as would result in nature from the stunting of the bud-axis (due directly to cold or indirectly to the advantage of protection gained thereby), attended with less, if any, stunting of the leaves. Then it is seen that the successive stages of condensation, beginning with the order $\frac{1}{2}$, will bring successively into contact with 0 (zero) the following numbers, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c., alternately to right and left, producing in succession

a series of orders which exactly resemble those found in nature, represented approximately by the successive terms of series A:—

$$\frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}, \frac{8}{21}, \frac{13}{34}, \text{ \&c.}$$

The first two or three stages of this process may be illustrated by mechanical experiment. Attach two rows of light spheres in alternate order on opposite sides of a stretched india-rubber band, give the band a slight twist, and relax tension; the system rolls up with strong twist into a tight complex order with three steep spirals, an approximation to the order $\frac{1}{3}$: if the spheres are set a little away from the axis, the order becomes condensed into (nearly) $\frac{2}{5}$, with five nearly vertical ranks; and it is plainly seen that further contraction, with increased distance of the spheres from the axis, will necessarily produce in succession the orders (nearly) $\frac{3}{8}$, $\frac{5}{13}$, $\frac{8}{21}$, &c., and that these successive orders represent successive maxima of stability in the process of change from the simple to the complex. These results are not invalidated by the consideration that the natural development of leaves is not simultaneous but successive.

By other diagrams it is shown that the same process of condensation operating on the orders represented by the lower fractions of series B $\left(\frac{1}{3}, \frac{1}{4}, \text{ \&c.}\right)$ will produce the higher orders of that series.

The same is also shown for series C $\left(\frac{1}{4}, \frac{1}{5}, \text{ \&c.}\right)$.

From the striking correspondence thus brought out between fact and theory, the conclusion is anticipated that we have here a clue to the secret of complex spiral leaf-order—that it is the result of condensation operating on some earlier and simpler order or orders, the successive stages of that condensation being ruled by the geometrical necessities of mutual accommodation among the leaves and axillary shoots under mutual pressure in the bud (taking the bud as the type of close-packed forms).

From this point of view, Hofmeister's law, that every leaf is found at that point in the circumference of the stem which has been left most open by the earlier leaves of the cycle, means that every leaf stands in that position relative to its neighbours which gave it most room for development in the bud.

Allusion was made above to deviation of leaf-ranks from the vertical as a necessary first step towards condensation. A series of six diagrams shows the gradual transition presented by different species of the South-African genus *Gasteria*, from a form in which the two ranks are exactly

vertical, to a form in which they are strongly twisted into a complex order with angular divergence nearly $\frac{3}{7}$, differing from $\frac{2}{5}$ by only $\frac{1}{35}$ of the circumference, and evidently admitting of further twist and closer approximation to the order $\frac{2}{5}$. From this striking series it is inferred that ranks originally vertical can and do acquire and transmit a tendency to deviate from the vertical, and that this tendency admits of augmentation to a high degree.

Assuming a twist, then, as a probable primary variation from an originally vertical condition of leaf-ranks, it is plain that each leaf would take a lower position, and the whole bud (with the same number of leaves) would be shorter, than in the untwisted form. The shorter bud, it is supposed, would have an advantage in cold seasons. The direct action of cold, by stunting the bud-axis (provided it did not stunt the leaves in the same proportion), would increase the twist. It may fairly be supposed that this twist would be taken advantage of and increased by natural selection in subservience to the close packing of the leaves. This course of modification is equivalent to the continued action of a force of vertical compression (mentioned above as the second requisite for condensation).

Transition similar to that in *Gasteria* is seen in the genus *Aloë*. Compare the two vertical ranks of *A. verrucosa* with the two twisted ranks of *A. obliqua*. In *A. serra* (Sachs, 'Lehrbuch der Botanik,' fig. 144) the change from the vertical to the strongly twisted form is found in the same plant: the basal leaves are in order $\frac{1}{2}$; the higher take complex order.

Exactly comparable (in this respect) with *Aloë serra* are the common laurel, Portugal laurel, Spanish chestnut, ivy, and others, which exhibit a similar change of leaf-order. These instances agree in presenting the complex order in the buds or parts of buds which occupy the most exposed situations, while they retain the simple order $\frac{1}{2}$ in the less exposed lateral buds or in their basal portion. The exposure in the former case may be regarded as a sample of that which, in the course of many generations, has (it is supposed) occasioned the condensation of leaf-order.

It is here contended that the force of gravity (to which the two-ranked leaf-order of lateral twigs is referred by some authors) could not have been equally the cause of the phenomena seen in the inclined lateral shoot of Spanish chestnut and in the upright *Aloë serra*: but the phenomena in the two cases are the same, and admit of a common explanation by the condensation theory, if we regard the basal portion

of the shoot as retaining the ancient order, and the more exposed terminal portion as having undergone protective modification.

The various degrees of obliquity of spiral ranks in the alternate orders of leaf-arrangement, and the complicated numerical relations existing between those various ranks, are all fully accounted for by the condensation theory.

Analyzing the spiral arrangement seen in a sunflower-head, a dandelion-head, a house-leek rosette, and an apple-twig, the result is found to be that any leaf (or fruit, in the first two instances), taken as zero, has for next neighbours successively, in rising steps of complexity of order, the 1st, 2nd, 3rd, 5th, 8th, 13th, 21st, 34th, 55th, 89th, 144th, &c. (in order of growth) alternately on the right side and on the left, producing alternately right- and left-handed spirals in sets of 1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c.; and these numbers are identical with those which would result from condensation of one of the lower orders of series A. Similar considerations apply to series B and C.

It is a significant relation that, in the sunflower and similar examples, the arrangement of the fruits in the composite head is such as would result from condensation of the arrangement of the leaves on the stem.

Among the *whorled* orders also there is equally strong evidence of the working of the same force of condensation.

First there is a series (α) derivable from the crucial arrangement. (This is shown by diagrams.) In the orders thus formed it is seen that conspicuous sets of parallel spirals will form the most striking feature, and that these spirals will be found in sets of 2, 4, 6, 10, 16, 26, 42, &c. (series α).

Instances are seen in the genera *Mercurialis* and *Sagina*, and the order *Dipsacaceæ*, in which last the whole series α finds exemplification.

Here also it is a significant relation that the fruit-order in the composite heads of *Dipsacaceæ* is such as would result from condensation of the crucial order of their stem-leaves. Some of these plants exhibit in their radical leaves a minor degree of the same condensation.

In like manner it is shown that condensation of whorls of three would produce orders with spirals in sets of 3, 6, 9, 15, 24, 39, 63, &c. (series β). For examples see Hofmeister, *op. cit.* p. 460.

Condensation (if any) of whorls of four would give spirals in sets of 4, 8, 12, 20, 32, &c. (series γ).

It is contended that the preceding evidence, drawn from both divisions of leaf-arrangement (alternate and whorled), is sufficient to establish the principle of condensation as having played an important part in the history of leaf-arrangement.

But there are phenomena in leaf-arrangement which are *not* explained by condensation. We have still to account for (1) the origin of alternate orders with 3, 4, 5, 7, 9, &c. vertical ranks; and (2) the origin of the different whorled orders, with whorls of two, three, four, five, &c. (with 4, 6, 8, 10, &c. vertical ranks).

The whole course of condensation depended on obliquity of ranks; but the distinguishing feature in these cases is that the ranks are exactly or almost exactly vertical.

All these cases are explained on the hypothesis that there has been in the vegetable kingdom a variability (*per saltum*) in the number of leaf-ranks; that a plant originally having two vertical ranks has, by a stroke of variation, produced shoots or seedlings with three vertical ranks; that three have varied to four, four to five, five to six, and so on; and that these "sports" have survived in some cases because of some advantage which they enjoyed (probably the same advantage as that gained by condensation—the accommodation of the same number of leaves in a shorter bud).

This hypothesis is supported by the variability which is found at the present day in the number of leaf-ranks in one and the same species. For instance, *Sedum sexangulare* exhibits *seven* nearly vertical ranks in order $\frac{2}{7}$, or *six* exactly vertical in whorls of three. *Fraxinus excelsa* has normally *four* exactly vertical ranks in whorls of two, but may be found with *five* nearly vertical ranks in order $\frac{2}{5}$, or with *six* exactly vertical in whorls of three. (These three varieties may be found on shoots growing from the same stump.) Whorls of three are often produced by plants usually bearing whorls of two (*e. g.* sycamore, lilac, laurustinus, maple, horse-chestnut, elder, ash, &c.), and whorls of four instead of three are seen in some species of *Sedum* and *Verbena.* Among these forms it does not seem possible that one could be produced from another by accumulative modification.

Professor Beal has found well-marked variation in the cones of larch, spruce, &c., the majority belonging to series A, but a considerable minority to series B or series *a*.

In dandelion-heads about 5 per cent. belong to series *a*.

Different species of the same genus (e. g. *Aloë verrucosa* and *variegata*, *Haworthia viscosa* and *pentagona*, and different species of *Sedum* and *Cactus*) often exhibit differences of leaf-order which can hardly be understood but as resulting from direct variation in number of leaf-ranks.

This hypothesis is also supported by analogy drawn from the animal kingdom. Among starfishes there is variability in the number of rays: *Asterias rubens* has sometimes four or six instead of five; *A. papposa* has from twelve to fifteen. Among mammals there is some variability in the number of digits.

Supposing, then, that, by strokes of variation, forms have been produced with (2) 3, 4, 5, 6, &c. vertical leaf-ranks, it is next to be considered how the arrangement of the leaves in each form would be affected by the demands of economy of space and mutual accommodation of ranks, supposing the ranks to be similar in point of size and number of leaves.

Two vertical ranks would gain lateral accommodation by taking alternate order $\frac{1}{2}$. Under vertical condensation, with twist in either direction, they would give rise to the successive orders of series A. (Two ranks are found in uneconomical opposite order in the genus *Mesembryanthemum*. This arrangement would be prone to fall into crucial order under vertical compression.)

Three vertical ranks would, with least surrender of lateral accommodation, assume alternate order $\frac{1}{3}$ (illustrated by diagram). A slight twist in one direction (No. 3 towards No. 1) would allow perfect lateral accommodation. In three-ranked plants (e. g. *Carex* and *Alnus*) such twist is usually found. Vertical condensation operating on three ranks possessing this obliquity would produce subsequent orders of series A. If the obliquity were in the opposite direction (No. 3 towards No. 2), condensation would produce successive orders of series B.

Four vertical ranks would economically fall into crucial order, the members of each rank fitting into the intervals between those of its neighbours. Opposite members therefore would stand at the same height, and would occupy one and the same node; they would also divide the circumference equally, and would stand over the intervals of the next lower pair. This crucial order under vertical condensation would produce series α. In rare cases four ranks might assume an alternate order $\frac{1}{4}$. Vertical condensation of this order $\frac{1}{4}$ with twist (No. 4 towards No. 1) would produce series B; with opposite twist (No. 4 towards No. 3) it would produce series C.

Five vertical ranks would, with least surrender of lateral accommodation, assume alternate order $\frac{2}{5}$. A slight obliquity (No. 5 towards No. 2), such as is usually found in nature, would allow perfect lateral accommodation. Condensation would then produce further orders of series A. With opposite obliquity (No. 5 towards No. 3) a new series $\left(\frac{2}{5}, \frac{3}{7}, \frac{5}{12}, \frac{8}{19}, \text{\&c.}\right)$ would be produced. Five ranks might also take alternate order $\frac{1}{5}$, which, condensed, would give with one twist series C, with the other a new series $\frac{1}{5}, \frac{1}{6}, \frac{2}{11}, \frac{3}{17}$, &c.

Six vertical ranks would economically fall into whorls of three, the members of each whorl dividing the circumference equally, and standing over the intervals of the next lower whorl. Condensation would give

series β. If six ranks should fall into alternate order $\frac{1}{6}$, one obliquity would lead to a series $\frac{1}{6}, \frac{1}{7}, \frac{2}{13}, \frac{3}{20}$, &c., the opposite to a series $\frac{1}{6}, \frac{2}{11}, \frac{3}{17}$, &c.

Seven vertical ranks would take alternate order $\frac{2}{7}$, facilitated by obliquity. Condensation would give series B. (It is needless to follow other possible lines of condensation.)

Eight vertical ranks would fall into whorls of four, with the same general characters noted above in whorls of two and three. Condensation would give series γ.

Nine would give $\frac{2}{9}$. Condensation would produce series C.

Ten would give whorls of five.

Eleven would give $\frac{2}{11}$.

Twelve would give whorls of six.

Thirteen would give $\frac{2}{13}$; and so on.

Thus it appears that the *whorled* orders would naturally arise from economic arrangement of *even* numbers (except 2), and the *alternate* orders from economic arrangement of *odd* numbers (including also 2), of vertical ranks.

It also appears that, in the whorled division, the members of each whorl will divide the circumference of the stem equally, and that successive whorls will alternate in angular position.

It has already been shown that in the alternate division the spiral arrangement of the leaves, with angular divergence limited to certain series of fractional values (A, B, C, &c.), would follow on the hypothesis of condensation.

These are the "main facts of leaf-arrangement" set down on page 298 to be accounted for.

It is possible that all the varieties of leaf-order at present existing may have been derived from an original two-ranked arrangement, partly by variation in the number of leaf-ranks, and partly by vertical condensation of the orders so formed. This view is supported by

(1) the high probability that the simplest form has been the earliest;

(2) the prevalence of the two-ranked form among lower phanerogamous plants (e. g. *Gramineæ*);

(3) the numerous instances of transition from a two-ranked order at the base of a shoot to a more complex order in the higher parts;

(4) the prevalence of the two-ranked arrangement of rootlets on

roots, taken in connexion with their probable homology with lateral shoots (the three ranks of rootlets in *Polygonaceæ*, and the four in carrot and parsnep, illustrate variability in number of ranks);

(5) the two-ranked arrangement of leaves in the seeds of Monocotyledonous plants, as compared with the more condensed (though probably at first two-ranked) order in the more highly developed Dicotyledonous embryo.

Summary.—The author is led to suppose:—

I. That the original form of leaf-arrangement was two-ranked.

II. That this original two-ranked form gave rise to forms with 2, 3, 4, 5, 6, 7, &c. ranks, by "sporting," as opposed to any process of accumulative modification.

III. That of the orders so formed those with an even number of ranks (except 2) have, as a rule, assumed a *whorled* arrangement, and those with two or an odd number of ranks have assumed an *alternate* arrangement, under the need of lateral accommodation of ranks in the bud (taken as type of close-packed forms).

IV. That all these orders have been subject to vertical condensation, under the need of vertical economy of space in the bud (taken as type of close-packed forms).

V. (*a*) That such condensation, operating on a 2-ranked, or 3-ranked, or 5-ranked alternate order $\left(\frac{1}{2}, \frac{1}{3}, \frac{2}{5}\right)$, has produced subsequent orders of series A $\left(\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}, \frac{8}{21}, \frac{13}{34}, \frac{21}{55}, \frac{34}{89}, \frac{55}{144}, \text{\&c.}\right)$.

(*b*) That condensation of a 7-ranked $\left(\frac{2}{7}\right)$ or rarely of a 3- or 4-ranked $\left(\frac{1}{3}, \frac{1}{4}\right)$ alternate order has produced subsequent orders of series B $\left(\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}, \text{\&c.}\right)$.

(*c*) That condensation of a 9-ranked $\left(\frac{2}{9}\right)$ or rarely of a 4- or 5-ranked $\left(\frac{1}{4}, \frac{1}{5}\right)$ alternate order has produced subsequent orders of series C $\left(\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}, \text{\&c.}\right)$.

(*d*) That condensation of a 4-ranked whorled order (whorls of two) has produced successive orders of series α, with spirals in sets of 4, 6, 10, 16, 26, 42, &c.

(*e*) That condensation of a 6-ranked whorled order (whorls of three) has produced successive orders of series β, with spirals in sets of 6, 9, 15, 24, 39, &c.

(*f*) That condensation (if any) of an 8-ranked whorled order (whorls of four) would produce successive orders of series γ, with spirals in sets of 8, 12, 20, 32, &c. Higher numbers of ranks would lead to higher series.

III. "On the Improvement of the Spectroscope." By Thomas Grubb, F.R.S. Received April 30, 1874.

The importance, as an instrument of research, which the spectroscope has reached within a few years, renders any improvement therein a matter of general scientific interest. Hitherto it has been under a disadvantage, which, though slight in amount in those cases in which the dispersive power of the instrument is moderate, becomes a rather serious annoyance to the observer when a number of prisms are used in serial combination, and the curvature of the spectral lines is proportionally increased, and only to be restrained *in appearance* by using a narrow breadth of the spectrum.

I have lately thought of a very simple and practical remedy (which may indeed have occurred to others, but which I have not seen mentioned), whereby those lines are rendered palpably straight in a very large field; but previous to describing it, it is desirable to refer to a statement appearing in the 'Astronomical Notices' for last month (March), viz. that the spectral lines can be rendered perfectly straight simply by returning them (after their first passage through a series of prisms arranged for minimum deviation) by a direct reflection from a plane mirror; and, further, that this has been accomplished in a spectroscope in construction for the Royal Observatory.

Such a statement has, as might be expected, produced several inquiries; in one case the querist is much interested, viz. by having a very large spectroscope in hand which, from its construction, involves the question of straight or curved lines resulting. It therefore seems desirable to remove any illusion which may be entertained, by a short consideration of the economy of the spectroscope, so far as the question of curvature is concerned.

The curvature of the spectral lines may be considered a function of the dispersion of a prism; it (the curvature) not only always accompanies the dispersion, but, further, its character is always the same with respect to the dispersion—that is to say, the centre of curvature will be found invariably to lie in the same direction with respect to the direction of the dispersion, the lines being invariably concave towards that end of the spectrum having the more refrangible rays*. This (which admits of the clearest proof) is adequate to show the impossibility that, by any

* Professor Stokes has indeed investigated a form of compound prism in which the resulting lines are straight, and on the same principle we may combine prisms (using of course media of different optical powers) in which, with *a balance* of dispersion remaining, the curvature might be found reversed; but this does not affect the general law. The curvature in that compound prism (which was the result of various trials, and first used in the spectroscope of the Great Melbourne Telescope, and now, I apprehend, in pretty general estimation and use) probably has a less proportional curvature of the lines than the simple prism.

kind of inversion, whether by reflections or otherwise, we can neutralize the curvature while doubling the dispersion.

If we examine the spectrum, as produced by a series of prisms placed in the position of minimum deviation, we necessarily find that the lines of higher refrangibility, also their centres of curvature, lie towards the centre of the polygon which the prisms themselves affect; and if we arrest the rays at any part of the circuit, and reflect them directly back by a plane mirror, this reflection reverses (right for left) not only the direction of the centre of curvature of the lines, but also the direction of the spectrum itself, both which are consequently doubled in amount after the rays have performed the second, or return, passage through the prisms; or (conversely) if, after the first passage through the prisms, we reflect the rays so as to pass through a similar set in such manner as to neutralize the curvature of the first set, we shall find the resulting dispersion reduced to zero.

The writer of the article having alluded to a difference between the reflection as given by a plane mirror and a prism of (double) total reflection, it may be observed that, so far as the dispersion and curvature are concerned, the cases are practically identical, the difference being that, in the double reflection, there is a *vertical* inversion of the spectrum, which, however, produces no discernible effect in either the spectrum or curvature of the lines; and as the spectroscope constructed with the double reflecting prism is known to produce, with double dispersion, double curvature, we here have an additional proof, if such were required, that the single reflecting mirror does the same.

The remedy, or means of producing straight spectral lines, which I have alluded to, is simply that of constructing the "slit" with curved instead of rectilinear edges. There is but little practical difficulty incurred in construction, and no apparent objection to its use. It may be objected that for each variation of prism-power in use there should be a special slit. It is, however, only in spectroscopes arranged for high dispersion that the curvature becomes objectionable; in such there is seldom a change required, and a single slit of medium balancing-power would probably remove all practical difficulty, or objectionable curvature of the lines. I have found by trial that, when two compound prisms were in use, giving a dispersion from A to H of nearly 14°, the spectral lines were straight in a field of one degree, when the radius of curvature of the slit was made 1·25 inch.

[*Note on the above Paper.*

If a ray of light be refracted in any manner through any number of prisms arranged as in a spectroscope, undergoing, it may be, any number of intermediate reflections at surfaces parallel to the common direction of the edges of the prisms—or, more generally, if a ray be thus refracted

or reflected at the surfaces of any number of media bounded by cylindrical surfaces in the most general sense (including, of course, plane as a particular case), the generating lines of which are parallel, and for brevity's sake will be supposed vertical, and if α be the altitude of the ray in air, α', α'', ..., its altitudes in the media of which the refractive indices are μ', μ'', ..., then

(1) The successive altitudes will be determined by the equations

$$\sin \alpha = \mu', \sin \alpha' = \mu'', \sin \alpha'' = \ldots.,$$

just as if the ray passed through a set of parallel plates.

(2) The course of the horizontal projection of the ray will be the same as would be that of an actual ray passing through a set of media of refractive indices $\frac{\mu' \cos \alpha'}{\cos \alpha}$, $\frac{\mu'' \cos \alpha''}{\cos \alpha}$, ... instead of μ', μ'', ... As $\alpha' < \alpha$, the fictitious index is greater than the actual, and therefore the deviation of the projection is increased by obliquity.

These two propositions, belonging to common optics, place the justice of Mr. Grubb's conclusions in a clear light.—April 30, G. G. STOKES.]

May 7, 1874.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows :—

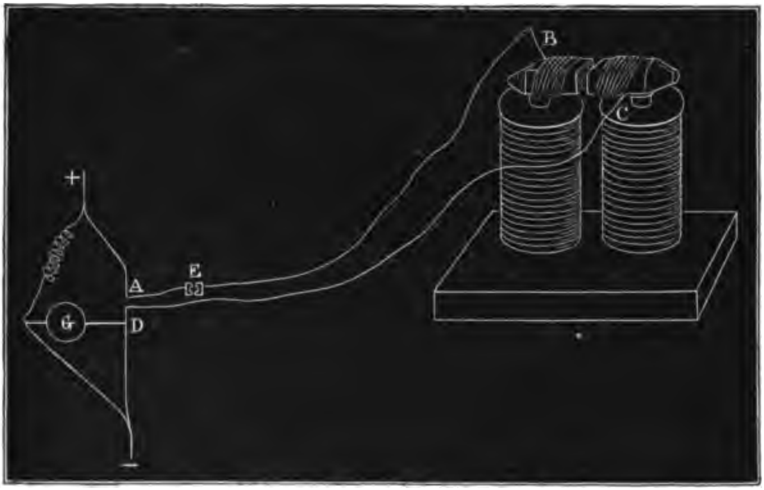
Isaac Lowthian Bell, F.C.S.
W. T. Blanford, F.G.S.
Henry Bowman Brady, F.L.S.
Thomas Lauder Brunton, M.D., Sc.D.
Prof. W. Kingdon Clifford, M.A.
Augustus Wollaston Franks, M.A.
Prof. Olaus Henrici, Ph.D.
Prescott G. Hewett, F.R.C.S.
John Eliot Howard, F.L.S.
Sir Henry Sumner Maine, LL.D.
Edmund James Mills, D.Sc.
Rev. Stephen Joseph Perry, F.R.A.S.
Henry Wyldbore Rumsey, M.D.
Alfred R. C. Selwyn, F.G.S.
Charles William Wilson, Major R.E.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Preliminary Experiments on a Magnetized Copper Wire." By Professor BALFOUR STEWART, LL.D., F.R.S., and ARTHUR SCHUSTER, Ph.D. Received March 30, 1874.

1. The following experiments were made in the Physical Laboratory of Owens College, Manchester. The copper wire employed (A B C D,



see fig.) was found to contain no perceptible trace of iron, nor was it sensibly magnetic, behaving quite in a neutral manner when tested by the highest magnetic power at our disposal. It was covered with gutta percha. The diameter of the wire was 0·0487 inch. The wire was wound fifty-three times, in one direction, round the poles of a powerful electromagnet, the length of wire encircling these poles being about twelve metres. The direct distance of the magnet from the galvanometer, G, was about twelve metres.

A Wheatstone bridge was employed, and a very delicate Thomson's reflecting galvanometer by Elliott Brothers, of which the resistance was 5540 B.-A. units. A circuit-breaker was placed in the circuit at E, close to the bridge. On some occasions, we used one consisting of a solid key, which might be removed, thus breaking the circuit; but, on other occasions, a fluid or mercurial circuit-breaker was employed.

When the left-hand pole of the electromagnet (see fig.) was made north the arrangement was called (1), and when the other pole was made north the arrangement was called (2). It will thus be seen, from the figure, that the current went round the magnet in the same direction as the molecular currents of arrangement (2).

Experiments were made at intervals of two minutes; and, on each occasion, the current was allowed to pass through the bridge for ten

seconds, the measurement being taken by the first swing of the galvanometer, which lasted for about eight seconds. Three cells of Grove's battery were used for producing this current; but, on the other hand, six similar cells were employed for magnetizing the electromagnet. The arrangements for magnetizing are not shown in the figure. The distance of the magnet was too great to affect the galvanometer-needle so as to alter its sensibility, the average deflection causing a difference in the zero of about four divisions of the scale.

2. In the first experiments made, the key at E (see fig.) was not taken out before the magnetism was put on or off, in consequence of which the induction-current, due to the wire coiled round the magnet, affected the galvanometer on these occasions; but, after December 12th, the key was taken out, so that no induction-current passed.

The following is a specimen of the observations made:—

December 17, 1873.

Time of putting on current.	Whole deflection observed (increasing deflection denotes increasing resistance in A B C D).	Condition of magnet.
11^h 11^m	312	off
13	317	off
15	311	off
17	345	(1)
19	328	off
21	306	(1)
23	303	off
25	293	(1)
27	300	off
29	290	(1)
31	307	off
33	283	(1)
35	292	off
37	288	(1)
39	302	off
41	292	(1)
43	309	off

It will be seen from this experiment that the *first effect* of putting on the magnetism was a marked increase of resistance; but, with this exception, the resistance, when the magnetism was on, was less than the mean of the two resistances on both sides of it, representing the magnetism off.

3. The arrangement remained untouched, as far as we know, from December 15, when it was finally made, until December 19, when the experiments were interrupted during the Christmas holidays; and in all

cases the *first effect* of putting on the magnetism was a marked increase of resistance. For instance, we have—

Date.	First off.	On first effect.	Second off.
Dec. 16	0	+36 on (2)	+ 3
„ 17	0	+34 on (1)	+17
„ 18	0	+54 on (1)	+24
„ 19	0	+33 on (1)	−18

It was soon seen that this *first effect* had some reference to the time elapsing since the last experiments were made. For instance, in the above Table, we see for December 18th a marked increase of resistance when the magnet was first put on; but, on the afternoon of that day, the experiments were repeated, and there was no apparent increase of resistance in this *first effect*. Next, with regard to the *average effect*: on Dec. 16th, 17th, and 18th, this *average effect* of magnetism was a decrease of resistance; but on Dec. 19th there was an apparent increase of resistance when the magnetism was on. We cannot say that nothing had been done to the arrangement between the 18th and 19th of December that might account for this change; but whatever was done must have escaped our recollection. Undoubtedly a good many experiments were made during the time between the 15th and 19th of December, and the direction of the magnetism was frequently changed. This curious anomaly, occurring unexpectedly, induced us to limit our future experiments to a definite set each day.

4. The experiments were resumed on January 7th, the arrangement having remained untouched during the holidays. From this date until January 10th inclusive, the key was taken out before beginning experiments in the morning: there was no peculiar *first effect*; while, on the other hand, an *average effect* denoting a decrease of resistance came out very prominently. On January 12th and 13th the key was only taken out before magnetizing, and on these occasions the *first effect*, denoting increased resistance, was sufficiently marked.

Our method of procedure was varied in the above manner up to January 27th; and it was invariably found that, whenever the key was taken out before commencing experiments, there was no *first effect*; but when it was kept in until before magnetizing, this *first effect* was sufficiently marked. These experiments concur in proving that the *first effect* has some reference to the previous treatment of the wire; but they do not prove that it is at the same time connected with the putting on of the magnetism. To determine this point we made a set of experiments on January 22nd, 26th, and 27th. When the current had become constant the key was taken out, but the magnetism was not put on; and on these occasions there was no *first effect* of the current upon itself in the direction of increased resistance, but rather in the opposite direction. It thus appears that the *first effect* which increases the resistance has not

only reference to the previous treatment of the wire, but depends also upon the magnetism being put on.

This result is confirmed by experiments made previous to Dec. 12th, in which the key was not taken out at all. For instance, we have on Dec. 9th,

First off.		On first effect.		Second off.
0	....	+54	....	+45

We have hitherto only spoken of the *first effect* obtained after January 7th; we now come to the *average effect.* From January 7th to January 27th inclusive, the magnetism was always put on in the direction (1), and the *average effect* invariably denoted a decrease of resistance when the magnetism was on.

5. On January 28th the magnetism was reversed; the effect during this day was very irregular. On January 29th, 30th, 31st, and February 2nd the key was left in until before magnetization. The *first effect* was now extremely large; but it was suspected that, during these experiments, the contact of the key was not very good.

On January 29th the *average effect* denoted a decrease of resistance, but on January 30th, 31st, February 2nd, 4th, 6th, the *average effect* denoted an increase of resistance.

6. From February 6th until February 11th the wires were left broken; on February 11th there was a very slight *first effect* in the direction of increased resistance, and a slight *average effect* in the direction of decreased resistance. On February 12th a mercury interruptor was used instead of a metal key, both the wires being broken by it, and its use was continued until February 18th. The interruptor was left in over night and the current was only broken before magnetization, but no *first effect* was observed.

From February 19th to February 26th one wire only was broken by the fluid interruptor; nevertheless there was no *first effect.*

On February 12th, when the fluid interruptor was first employed, there was a very small *average effect* in the direction of increased resistance; but, in all the experiments afterwards, this *average effect* was in the direction of decreased resistance. The magnetism had been in the direction (2) from January 28th; but, during the experiment of February 25th, it was reversed and retained in this condition through the experiment of February 26th, without appearing to affect the results.

7. From these experiments we may perhaps conclude as follows:—

In the *first place*, there is a *first effect* in the direction of increased resistance which appears to have reference to three things—namely, the previous state of the wire, the solidity of the circuit, and its magnetization.

In the *second place*, we have an *average effect*, of which the normal state appears to denote a decreased resistance while the magnetism is on, without reference to the direction of the magnetism.

In the *third place*, when, in a solid circuit, the direction of the magnetism has been recently changed, there appears to be a temporary reversal of the average effect, which appears, at first, as an increase of resistance. Besides the evidence herein detailed, we have other evidence in favour of the third conclusion; for in some preliminary experiments, in which we frequently reversed the poles, we found an increase of resistance when the magnetism was on. We have given, in a Table appended to this paper, a synopsis of our various experiments.

8. We are led to conclude, from other experiments besides these, that the effect of the magnetism is not merely confined to the part of the copper wire wound round the poles, but is propagated all along the wire. On December 2nd, for instance, the current was passed through the wire, the galvanometer being joined as a secondary circuit. The main current was therefore measured.

The deflections were as follows:—

297	off	300	off
300	(1)	302	(1)
297	off	301	off
300	(1)		

This shows an average strengthening of the current, equal to about one two-hundredth part of the whole. Were this strengthening due to merely the change of resistance of that part of the wire wound round the poles, the effect, as measured by the much more delicate arrangement of Wheatstone's bridge, would be much larger than was actually observed.

9. Allusion was made in article 7 to some preliminary experiments, in which increased resistance was observed when the magnetism was put on (1) and (2) alternately. Similar experiments were made, giving the same result with a piece of coke and graphite, which were placed between the poles of the magnet.

10. We have also some evidence that a copper wire, one end of which is wound round the pole of the magnet, changes its position in the electromotive series. Two copper wires were dipped into dilute nitric acid and connected with the galvanometer. A weak current passed through the galvanometer owing to a slight difference in the copper wires, one of which was also connected with the copper wire wound round the magnet. When the magnet was on, the current, as a rule, changed in intensity; but the effect was small, and the difficulty of having two copper wires which, when joined together and dipped into nitric acid, give a current sufficiently weak and constant, prevented us from getting any decided results.

11. In conclusion we have to state that we regard these results which we have ventured to bring before the Royal Society as preliminary, the correctness of which will, we trust, be confirmed by the further experiments which it is our intention to make.

Date. 1873.	Nature of Experiment.	Value of first effect.	Number of observations.	Mean effect, excl. of first effect (— denote decrease of resistance).
Dec. 17.	Metal key left in over night taken out before magnetism was put on (1)	off. on. off. 0+34+17	15	−14
„ 18.	Metal key left in over night taken out before magnetism was put on (1)	0+54+24	30	− 9
„ 19.	Metal key left in over night taken out before magnetism was put on (1)	0+33−18	15	+21
1874. Jan. 7.	The key was taken out before beginning the experiments in the morning	No first effect.	15	−17
„ 8.	The key was taken out before beginning the experiments in the morning	Ditto	15	−19
„ 9.	The key was taken out before beginning the experiments in the morning	Ditto	15	−13
„ 10.	The key was taken out before beginning the experiments in the morning	Ditto	15	−20
„ 12.	The key was not taken out until before magnetizing	0+47+18	15	−18
„ 13.	The key was taken out (same as Jan. 7)	No first effect.	Irregular	action.
„ 14.	The key was not taken out (same as Jan. 12)	0+3−6	15	−19
„ 15.	Ditto	0+17+11	15	− 2
„ 16.	The key was taken out (same as Jan. 7)	No first effect.	15	−67
„ 17.	The key was not taken out (same as Jan. 12)	0+28+119	15	−22
„ 20.	The connexions had been broken since Jan. 17	No first effect.	15	−44
„ 21.	The key was taken out (same as Jan. 7)	0+7+1	15	− 9
„ 22.	When the current was constant the key was taken out, but the magnet was not put on before the current had again become constant	No first effect.	15	−13
„ 24.	The key had been left out since Jan. 22	Ditto	15	−11
„ 26.	Same as Jan. 22	Ditto	15	−33
„ 27.	Ditto	Ditto	15	−23
„ 28.	The magnet was put on (2) from this day until Feb. 25	......	......	......
„ 29.	The magnet was put on (2), key left in (same as Jan. 12)	0+102+47	15	−23
„ 30.	The magnet was put on (2), key left in (same as Jan. 12)	0+219	15	+25
„ 31.	The magnet was put on (2), key left in (same as Jan. 12)	0+137+161	15	+16
Feb. 2.	The magnet was put on (2), key left in (same as Jan. 12)	0+ 47+84	15	+11
„ 3.	....	......	......	......
„ 4.	The key was taken out and put in after several offs, same as Jan. 22	No first effect.	15	+31
„ 5.	....	......	......	......
„ 6.	The key was taken out (same as Jan. 22)	No first effect.	15	+ 3
„ 11.	....	0+7+3	15	− 3
„ 12.	A mercury interruptor was used instead of metal key	No first effect.	31	+ 3
„ 13.	The mercury interruptor was kept in over night	Ditto	15	−11
„ 14.	The mercury interruptor was kept in over night	Ditto	15	−36
„ 16.	New mercurial contact breaker from Elliott Brothers used	Ditto	15	−18
„ 17.	Ditto key left in (same as Jan. 12)	Ditto	15	− 5
„ 18.	Ditto Ditto.	Ditto	15	− 7
„ 19.	One wire only was broken	0+11+16	15	− 2
„ 20.	One wire only was broken, key left in (same as Jan. 12)	No first effect.	15	−13
„ 22.	One wire only was broken, key left in (same as Jan. 12)	Ditto	17	−12
„ 24.	One wire only was broken, key left in (same as Jan. 12)	Ditto	17	− 8
„ 25.	One wire only was broken, magnet on (1)	Ditto	15	− 4

REMARKS.

Dec. 18, 1873. For a second series of 15 on this day no first effect was found.

Jan. 15, 1874. There was a sudden change of the current during the experiments, to which the unusually small effect is most likely due.

Jan. 16. There was a sudden change of the current during the experiments, to which the unusually large effect is most likely due.

Jan. 17. There was an irregularity at the beginning of the experiment.

Jan. 20. Action somewhat irregular.

Jan. 22. There seemed to be a first effect of the current on itself in the opposite direction, 0—14—9.

Jan. 26. There seemed to be again a first effect in opposite direction, 0—47—57.

Jan. 27. Ditto Ditto 0—17—17.

Jan. 28. The action was very irregular.

Jan. 29. It is suspected that during the experiments from Jan. 29 to Feb. 12 the contact at the key was not very good.

Feb. 3. The action was very irregular.

Feb. 4. There seemed to be two first effects of the current upon itself in the direction of increased resistance.

Feb. 5. The action was very irregular.

Feb. 6. There seemed to be a first effect of decreased resistance of current upon itself.

Feb. 11. The wires had been broken since Feb. 6th.

Feb. 12. One of the wires had got between the pole and the core of the magnet.

Feb. 24. After the first on (2) the magnet was always put on (1).

II. "Note on some Winter Thermometric Observations in the Alps." By E. FRANKLAND, F.R.S.

During the past winter, I spent a fortnight at the village of Davos, Canton Graubünden, Switzerland, and had thus an opportunity of experiencing some of the remarkable peculiarities of the climate of the elevated valley (the Prättigau) in which Davos is situated. The village has of late acquired considerable repute as a climatic sanitarium for persons suffering from diseases of the chest. So rapidly has its reputation grown, that while in the winter of 1865–66 only eight patients resided there, during the past season upwards of three hundred have wintered in the valley.

The summer climate of Davos is very similar to that of Pontresina and St. Moritz, in the neighbouring high valley of the Engadin—cool and rather windy; but so soon as the Prättigau and surrounding mountains become thickly and, for the winter, permanently covered with snow, which usually happens in November, a new set of conditions come into play and the winter climate becomes exceedingly remarkable. The sky is, as a rule, cloudless or nearly so; and, as the solar rays, though very powerful, are incompetent to melt the snow, they have little effect upon the temperature, either of the valley or its enclosing mountains; consequently there are no currents of heated air; and, as the valley is well sheltered from more general atmospheric movements, an almost uniform calm prevails until the snow melts in spring.

According to Dufour's trigonometrical measurements, Davos is 1556 metres, or 5105 feet, above the sea; the measurements of the Swiss Meteorological Society make the height 1650 metres, or 5413 feet; and my own estimation with an aneroid gave it as 4000 feet above Zürich, or 5352 feet above the sea. The village of Davos is therefore about 500 feet lower than the summit of the Rigi.

I arrived on the evening of the 20th of December, and found the snow lying from two to three feet deep on the flat sole of the valley. On the following morning the thermometric observations were commenced with instruments supplied to me by Mr. L. Casella, all of which had been certified at the Kew Observatory. For the corresponding readings at Greenwich I am indebted to Mr. Glaisher.

December 21st, 1873.—From behind the sharp peak of the Schwarzhorn the sun rose at the Seehof Hotel, Davos-Dörfli, at 8.35 A.M. Throughout the day the sun was alternately clear and obscured by clouds. At Davos-Platz it did not rise until 9.44 A.M. At 10 A.M. the mercurial thermometer with blackened bulb *in vacuo* showed 44° C. (111°·2 Fahr.) in the sunshine, and 45° C. (113° Fahr.) at 2.50 P.M. At Greenwich the readings on this day with the blackened bulb *in vacuo* placed on the grass * in the sunshine were:—at 9 A.M., 9°·3 C. (48°·7 Fahr.), at noon and at 3 P.M., 21°·9 C. (71°·5 Fahr.), the maximum during the day being 21°·9 C. (71°·5 Fahr.). The maximum temperature observed in the shade was 10°·9 C. (51°·7 Fahr.), and the minimum on grass in the shade 2°·1 C. (35°·7 Fahr.).

December 22nd.—A mercurial thermometer with black glass bulb was laid on the snow at 8 A.M.; twenty minutes later, or fifteen minutes before sunrise, it marked −18°·3 C. (−1° Fahr.). The sky was deep blue, and almost perfectly cloudless during the whole day. Five minutes after sunrise many of the patients at the Seehof Hotel were walking in the open air without any special wraps, and many of them without overcoats. In the brilliant sunshine one felt comfortably warm sitting in front of the hotel in a light morning coat. The following thermometrical observations were made on this day:—

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* Since the above was written I have ascertained that the readings of this kind of instrument are much higher when it is laid on grass than when it is clamped upon a staff at a height of 5 feet above the ground. Thus, at St. Leonard's-on-Sea on the 7th of April last, this thermometer in sunshine stood at 42°·3 C. at 11.50 A.M., when placed 5 feet from the ground, but when laid on the grass it promptly rose to 56°·5 C. It is therefore evident that the readings of the solar thermometer at Greenwich, given throughout this paper, are much too high for fair comparison with the Davos temperature, the thermometer at Greenwich having been always laid upon the grass. On the 7th of April the sky at St. Leonard's was clear, the air warm with but little wind, and the sun bright; nevertheless the maximum temperature during the day in sunshine was 2°·7 C. lower than that observed with the same instrument at Davos on the 21st of December last.—May 7, 1874.

I. Blackened bulb *in vacuo*. In sunshine.

8.45 A.M.	8.50 A.M.	9.0 A.M.	9.45 A.M.	10.15 A.M.	10.45 A.M.	11.15 A.M.	Noon.	Light cloud, 12.40 P.M.	Clear, 1.45 P.M.
22°·0 C.	28°·0 C.	30°·0 C.	37°·3 C.	39°·3 C.	39°·5 C.	41°·2 C.	42°·4 C.	37°·2 C.	43°·0 C.

This thermometer was clamped to an alpenstock at a height of about five feet from the snow in all the observations recorded in this paper. At Greenwich the readings were, with blackened bulb *in vacuo* :—maximum 12°·8 C. (55° Fahr.); at 9 A.M., 8°·5 C. (47°·3 Fahr.); at noon and at 3 P.M., 12°·8 C. (55° Fahr.). The maximum in the shade was 10°·4 C. (50°·7 Fahr.), and the minimum on grass in the shade −1°·7 C. (28°·9 Fahr.).

II. Plain mercurial thermometer with black glass bulb. In sunshine.

9.45 A.M.	10.15 A.M.	11.15 A.M.	Noon.	1.45 P.M.
−1° C.	0°·6 C.	3°·3 C.	3°·3 C.	7°·2 C.

III. Plain mercurial thermometer with black glass bulb. In shade.

10.15 A.M.	11.15 A.M.	Noon.	1.45 P.M.
−4°·0 C.	−1°·0 C.	−1°·0 C.	−2°·0 C.

IV. Plain mercurial thermometer with black glass bulb, placed in a box lined with padded black cloth and covered with plate-glass ¼ inch thick.

9.45 A.M.	10.15 A.M.	Noon.	12.35 P.M.	2 P.M.
75°·0 C.	85°·0 C.	100°·0 C.	102°·8 C.	105°·0 C.

Thus in mid winter the unconcentrated solar rays at Davos are capable of producing, under favourable circumstances, a temperature of 221° Fahr.,—9° Fahr. above the boiling-point of water at the sea-level, or 21° Fahr. above that point at Davos, where I found water to boil at 200° Fahr. when the barometer stood at 627·3 millims.

December 23rd.—The sky was again deep blue and cloudless nearly the whole of the day. The atmospheric pressure was 627·3 millims., and the temperature eight minutes before sunrise, as shown by a black-glass-bulb thermometer laid upon the snow, was again $-18°·3$ C. ($-1°$ Fahr.). The following thermometric observations were made :—

I. Blackened bulb *in vacuo*. In sunshine.

90 A.M.	9.30 A.M.	11.0 A.M.	11.15 A.M.	11.30 A.M.	12.15 P.M.	2.0 P.M.	Light clouds, 2.23 P.M.
28°·5 C.	35°·5 C.	37°·2 C.	39°·0 C.	39°·0 C.	39°·6 C.	40°·0 C.	34°·0 C.

II. In the shade, the plain mercurial thermometer, with black glass bulb, stood at $-9°·4$ C. (15·1° Fahr.) at 11.30 A.M. It was freely suspended in the air at a height of about three feet from the snow.

At Greenwich the readings were, with blackened bulb *in vacuo* :—maximum 22°·8 C. (73° Fahr.); at 9 A.M., 4°·4 C. (40° Fahr.); at noon, 12°·6 C. (54°·6 Fahr.); at 3 P.M., 22°·8 C. (73° Fahr.). The maximum in the shade was 8°·3 C. (46°·9 Fahr.), and the minimum on grass in the shade $-2°·3$ C. (27°·9 Fahr.).

December 24th.—As the Fluela pass, the highest carriage-road in Switzerland, was still open for sledges, I determined to make some observations on the summit, which is 7890 feet above the sea, and consequently about 2538 feet above Davos. Starting from Davos at 8 A.M., I arrived at the summit of the pass, where there is a small hotel and telegraph station, at 10.30 A.M.

The early morning was somewhat cloudy, but, about ten o'clock, the sky became perfectly clear and deep blue, and continued so until the sun set behind the Schwarzhorn, a few minutes past noon. The following temperatures were recorded :—

I. The blackened bulb *in vacuo* marked 41°·7 C. at 11 A.M. in the sunshine, 42°·3 C. at 11.30 A.M., and 42°·3.C. at 12 o'clock.

II. The plain black glass bulb in the shade showed at noon $-7°·2$ C. when freely suspended about two feet above the snow in a brisk breeze.

The highest temperature in sunshine which I have observed at Davos at noon, with the blackened bulb *in vacuo*, was 42°·5, which scarcely differs from that read on the Fluela pass at the same hour. Sò far as these limited observations go, therefore, they indicate that the solar rays are not of appreciably higher thermal intensity at a height of 7890 feet than at a height of 5350 feet. I may add that the thermometer in the sunshine was sheltered from the wind on the Fluela pass, and was, in all respects but one, in a more favourable position for attaining a high temperature than at Davos. The one unfavourable condition was its exposure to less solar heat reflected from the snow than at Davos.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 19°·5 C. (67°·1 Fahr.); at 9 A.M., 9°·6 C. (49°·3 Fahr.); at noon, 18°·6 C. (65°·5 Fahr.); and at 3 P.M., 19°·5 C. (67°·1 Fahr.). The maximum in the shade was 10°·5 C. (50°·9 Fahr.), and the minimum on grass in the shade −3°·1 C. (26°·5 Fahr.).

December 25*th*.—The sky was again deep blue and perfectly cloudless. The air was also apparently clear, except at about 9 A.M., when the village and valley became immersed in a light fog, which consisted of minute snow crystals. On this and most subsequent days isolated crystals could be distinctly seen floating in the air, by placing the eye in shadow and then looking into the sunshine. The abundance or paucity of these suspended and, under ordinary circumstances, invisible snow crystals must exercise a powerful influence upon the intensity of solar radiation. To this cause, for instance, it was probably due that at 1.45 P.M. on this day, although the sky was perfectly clear and the sunshine most intensely brilliant, the blackened bulb *in vacuo* only stood at 35° C. in the sun, whereas at noon, when all the conditions were apparently the same (except, of course, the sun's altitude), the temperature was 5° C. higher. The following readings were taken:—

I. Blackened bulb *in vacuo*. In sunshine.

9.0 A.M., frozen fog.	9.15 A.M., clear.	10.20 A.M., clear.	11.15 A.M., clear.	Noon, clear.	1.45 P.M., clear.
22°·5 C.	32°·5 C.	37°·9 C.	39°·2 C.	40°·0 C.	35°·0 C.

II. The black glass bulb on the snow eight minutes before sunrise marked −12°·8 C. At noon in the shade it stood at −9°·1 C. Height of barometer 630 millims.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 10°·4 C. (50°·8 Fahr.); at 9 A.M., 4°·6 C. (40°·3 Fahr.); at noon and at 3 P.M., 10°·4 C. (50°·8 Fahr.). The maximum in the shade was 7°·5 C. (45°·5 Fahr.), and the minimum on grass in the shade −2°·7 C. (27°·2 Fahr.).

December 26*th*.—Not the smallest cloud was visible during the whole of this day. The sky was intensely blue and the air perfectly calm. Atmospheric pressure 630 millims. Fifteen minutes before sunrise the thermometer on the snow marked −16°·7 C. At 1.50 P.M. the same thermometer in the shade stood at −4°·1 C. The following readings in the sunshine were made with the blackened bulb *in vacuo*:—

8.45 A.M.	9.0 A.M.	10.0 A.M.	10.30 A.M.	11.0 A.M.	11.30 A.M.	Noon.	12.30 P.M.	1.0 P.M.	2.30 P.M.	2.50 P.M.
22°·0 C.	31°·8 C.	39°·8 C.	40°·8 C.	42°·5 C.	43°·7 C.	42°·5 C.	42°·7 C.	42°·0 C.	31°·0 C.	33°·1 C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 8°·8 C. (47°·9 Fahr.); at 9 A.M., 6°·7 C. (44° Fahr.); at noon and at 3 P.M., 8°·8 C. (47°·9 Fahr.). The maximum in the shade was 8°·2 C. (46°·7 Fahr.), and the minimum on grass in the shade 4° C. (39°·2 Fahr.).

December 27th.—A cloudless morning and deep blue sky. Eight minutes before sunrise the thermometer on the snow indicated −17°·2 C. At 10.25 A.M. the black bulb *in vacuo* registered in the sunshine 36°·5 C. and at noon 38°·5 C. The afternoon was cloudy and no observations were made.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 13°·6 C. (56°·4 Fahr.); at 9 A.M., 7°·5 C. (45°·5 Fahr.); at noon 6°·2 C. (43°·1 Fahr.); and at 3 P.M. 13°·6 C. (56°·4 Fahr.). The maximum in the shade was 8°·4 C. (47°·2 Fahr.), and the minimum on grass in the shade −3°·7 C. (25°·3 Fahr.).

December 28th.—At 4.30 A.M. there was a violent storm of wind with snow; afterwards moderate wind with snow until the afternoon. The barometer stood at 615 millims. At 2 P.M. the blackened bulb *in vacuo* registered 28° C. in sunshine.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 0°·7 C. (33°·2 Fahr.); at 9 A.M., −0°·5 C. (31°·1 Fahr.); at noon and at 3 P.M. 0°·7 C. (33°·2 Fahr.). The maximum in the shade was 0°·6 C. (33° Fahr.), and the minimum on grass in the shade was −8°·4 C. (16°·9 Fahr.).

December 29th.—Sky deep blue and quite free from cloud during the whole day. Barometer 620 millims. At 8 A.M. the thermometer on the snow stood at −22°·2 C. A spirit thermometer (not verified), 4 feet from the ground, indicated −22°·1 C. At noon the thermometer in the shade stood at −18°·1 C. The following observations were made with the blackened bulb *in vacuo*:—

9.0 A.M.	10.0 A.M.	11.0 A.M.	11.30 A.M.	Noon.	4 minutes after sunset, 3.30 P.M.
18°·0 C.	30°·0 C.	33°·7 C.	37°·0 C.	33°·7 C.	−12°·0 C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 28°·4 C. (83°·2 Fahr.); at 9 A.M., −1°·6 C. (29°·2 Fahr.); at noon, 28°·3 C. (82°·9 Fahr.); and at 3 P.M., 28°·4 C. (83°·2 Fahr.). The maximum in the shade was 4°·2 C. (39°·5 Fahr.), and the minimum on grass in the shade was −9°·6 C. (14°·8 Fahr.).

December 30th.—Sky deep blue and perfectly free from cloud during the whole day. Barometer 621·7 millims. At 8 A.M. the thermometer on the snow stood at −26°·4 C. (−15°·5 Fahr.). A self-registering minimum

spirit thermometer (unverified), fixed to a post 4 feet above the snow, recorded −18° Fahr. as the minimum temperature during the night of December 29–30th. At 2 P.M. the thermometer in the shade stood at −12°·8 C. The air was apparently equally clear throughout the whole day. The following readings of the blackened bulb *in vacuo* in sunshine were made:—

9.0 A.M.	9.30 A.M.	10.0 A.M.	11.30 A.M.	12.15 P.M.	1.30 P.M.	2.0 P.M.
25°·5 C.	32°·3 C.	35°·0 C.	37°·5 C.	35°·2 C.	38°·5 C.	33°·7 C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 22°·9 C. (73°·2 Fahr.); at 9 A.M., 2°·7 C. (36°·9 Fahr.); and at 3 P.M., 22°·9 C. (73°·2 Fahr.). The maximum in the shade was 7°·5 C. (45°·5 Fahr.), and the minimum on grass in the shade was −4°·9 C. (23°·1 Fahr.).

December 31*st*.—Sky deep blue, sun quite free from clouds during the whole day. Very light streaks of cloud appeared in the S.W. just before sunset. Barometer 621·5 millims. At 8 A.M. the thermometer on the snow registered −23°·6 C.; at noon the thermometer in the shade stood at −10°·C. A naked thermometer with smoked black glass bulb freely suspended registered only −2°·8 C. at 9.30 A.M. in sunshine. During the day abundance of snow crystals were frequently observed to be floating about in the air. The blackened bulb *in vacuo* was read in the sunshine as follows:—

9.30 A.M.	10.0 A.M.	11.0 A.M.	Noon.	12.30 P.M.	2.0 P.M.	2.50 P.M.
32°·9 C.	36°·5 C.	38°·7 C.	39°·0 C.	40°·0 C.	35°·0 C.	21°·5 C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 24°·4 C. (76° Fahr.); at 9 A.M., 8°·1 C. (46°·6 Fahr.); at noon, 21°·3 C. (70°·4 Fahr.); and at 3 P.M., 24°·4 C. (76° Fahr.). The maximum in the shade was 10°·4 C. (50°·7 Fahr.), and the minimum on grass in the shade was 0°·6 C. (33°·1 Fahr.).

January 1*st*, 1874.—A cloudy morning. Sun only slightly visible before 9 A.M.; afterwards brilliant between the clouds. Barometer 625 millims. At 8.15 A.M. the thermometer on the snow marked −13°·9 C., and the unverified self-registering minimum −17°·3 C. At 11.30 A.M. the thermometer in the shade stood at −3°·3 C. The following readings of sunshine temperatures were made with the blackened bulb *in vacuo*:—

9.0 A.M., cloudy.	9.30 A.M., slight cloud over sun.	9.45 A.M., sun clear, rest of sky cloudy.	10.0 A.M., clear.	10.30 A.M., cloudy.	11.30 A.M. cloudy.	12.30 P.M., cloudy.
$-1°·0$ C.	30°·5 C.	43°·5 C.	44°·0 C.	21°·3 C.	18°·5 C.	11°·5 C., had been 23° C. since 11.30 A.M.

The afternoon and night were cloudy.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 19°·6 C. (67°·3 Fahr.); at 9 A.M., 2°·8 C. (37° Fahr.); at noon and at 3 P.M., 19°·6 C. (67°·3 Fahr.). The maximum in the shade was 8°·1 C. (46°·6 Fahr.), and the minimum on grass in the shade was $-1°·2$ C. (29°·9 Fahr.).

January 2nd.—A cloudy morning. Sun not visible until nearly 9 A.M.; afterwards clear and calm, except at about 10.40 AM., when a few light clouds appeared. Minimum temperature during the night, as measured by an unverified spirit thermometer, $-9°·2$ C. At 8 A.M. the thermometer on the snow stood at $-6°·7$ C.; atmospheric pressure 627·8 millims. At noon the thermometer in the shade stood at $-5°$ C., and at 3 P.M. it registered $-4°·6$ C. The following observations were made with the blackened bulb *in vacuo*:—

9.0 A.M.	9.15 A.M.	10.0 A.M.	10.30 A.M.	10.40 A.M.	Noon.	12.30 P.M.	1.30 P.M.	3.0 P.M.
29° C.	38° C.	40° C.	41° C.	31°·5 C.	43° C.	40° C.	41° C.	27°·5 C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 14°·2 C. (57°·5 Fahr.); at 9 A.M., 9°·3 C. (48°·8 Fahr.); at noon and at 3 P.M., 14°·2 C. (57°·5 Fahr.). The maximum in the shade was 10°·4 C. (50°·7 Fahr.), and the minimum on grass in the shade was 2°·6 C. (36°·6 Fahr.).

January 3rd.—A calm but cloudy morning. At sunrise the thermometer on the snow registered $-6°·9$ C. The unverified spirit minimum showed the lowest temperature during the night to have been $-11°$ C. Barometer 624 millims. At 11 A.M. the sun was just visible, and in the afternoon the clouds became still thinner. At 12.15 P.M. the thermometer in the shade stood at $+0°·8$ C. The blackened bulb *in vacuo* stood at 9° C. at 9 A.M. and also at 11 A.M. Between 11 and noon it rose to 29° C. At 12.15 P.M. it marked 15°·5 C., and between that hour and 2 P.M. it reached 28° C., whilst at 2 P.M. it stood at 25° C.

At Greenwich the readings were, with blackened bulb *in vacuo*:—maximum 23°·8 C. (74°·9 Fahr.); at 9 A.M., 7°·2 C. (44°·9 Fahr.); at noon 10°·4 C. (50°·8 Fahr.); and at 3 P.M., 23°·8 C. (74°·9 Fahr.). The maximum in the shade was 9°·2 C. (48°·6 Fahr.), and the minimum on grass in the shade was −·4° C. (31°·3 Fahr.).

During the winter of 1870–71 a series of meteorological observations were made at Davos by Mr. Arthur Wm. Waters, F.G.S., but I am not aware whether the instruments used were verified. The minimum temperatures observed with a Hermann's metallic spiral thermometer were:—

	At Davos.	Corresponding temperature at Greenwcih.
November, 1870	−10°·7 C.	− 5°·5 C.
December, 1870	−29°·5 C.	−15°·7 C.
January, 1871	−20°·7 C.	−11°·1 C.
February, 1871	−18°·7 C.	− 5°·0 C.

The maximum sun-temperatures observed with a blackened bulb *in vacuo* were:—

	At Davos.	Corresponding temperature at Greenwich.
November, 1870	46°·3 C.	35°·1 C.
December, 1870	46°·1 C.	26°·0 C.
January, 1871	47°·3 C.	26°·6 C.
February, 1871	52°·2 C.	38°·8 C.

The chief remarkable things about the observations made last winter are, first, the very high sun-temperatures prevailing contemporaneously with very low air- or shade-temperatures, and secondly, the comparative uniformity of the solar heat from sunrise to sunset. Thus on the 29th of December, whilst the temperature of the air was −18°·1 C., the sun-thermometer stood at +37° C., and on the following day, with an air-temperature not exceeding −12°·8 C., the sun-temperature was 38°·5 C. Again, the sun-temperatures observed on the 26th of December illustrate the comparative uniformity of solar radiation during the day, when the sky remains cloudless. Twenty-five minutes after sunrise the solar thermometer indicated 31°·8 C.; at noon it stood at 42°·5 C., and at thirty-five minutes before sunset it recorded 33°·1 C.

Besides the intensity of solar radiation and its comparative uniformity during the day, the rarity and calmness of the air are important factors amongst the causes of the peculiar climate of Davos. With the barometer standing at 615 millims. the weight of air in contact with a given surface of the skin is about one fifth less than it is at the sea-level. The excessive dryness of the air at Davos has probably but little special influence upon the sensation of heat and cold, because the maximum proportion of aqueous vapour present in air near 0° C. is everywhere small, and the specific heats of equal volumes of air and aqueous vapour are not widely

different. On the other hand, the absence of suspended watery particles in the air has, no doubt, very considerable influence in preventing the chilling of the skin. Not only are such liquid particles present when there is visible fog, but they often exist in great numbers when the air possesses its usual transparent appearance. Another very important influence upon the sun-temperature is the reflection of solar rays from the snow. The valley of Davos is about one mile wide, and has precipitous sides and a flat sole. The villages of Davos-Dörfli and Davos-Platz are situated on the north-west slope of the valley, and consequently receive the scattered solar rays reflected from a large area of snow. I have no doubt that the sun-temperature at the opposite side of the valley is markedly lower; but having no second sun-thermometer, I could not ascertain this by the comparison of simultaneous thermometric observations. When staying at Ventnor, in the winter of 1872–73, I noticed that a not inconsiderable proportion of the total solar heat falling upon a house on a cliff, near the shore, was reflected from the sea. M. Dufour has since observed the same phenomenon between Lausanne and Vevay on the Lake of Geneva*, and has actually measured the proportions of direct and reflected heat incident at five different stations on the northern shore of the lake. He found that the proportion of reflected heat was as much as 68 per cent. of the heat directly incident from the sun, when the sun's altitude was between 4° 38′ and 3° 34′. At about 7° altitude the proportion was between 40 and 50 of reflected to 100 of direct heat. Even at about 16° altitude the proportion was between 20 and 30 of reflected to 100 of direct heat; but when the sun was higher than 30°, the reflected heat was hardly appreciable. It will be seen that this action of extensive reflecting surfaces of snow or water must exert a powerful influence upon the maximum temperature of places favourably situated for receiving the reflected rays; and, moreover, where the proportion of heat reflected varies (as it has been proved to do in the case of water, and as it doubtless also does in the case of snow) inversely as the angle formed by the incident rays and the reflecting surface, this action must materially contribute, especially in winter, to the maintenance of an approximately uniform sun-temperature throughout the day. At Davos and similar elevated stations, however, the comparative freedom of the air from suspended liquid and solid particles must obviously contribute, to a still greater extent, to such a result; for as pure and dry air is transcalent and reflects light but slightly, the horizontal sunbeams, passing through such air, would be nearly as powerful as vertical rays.

The peculiar winter climate of Davos appears, therefore, to depend upon the following conditions:—

1. *Elevation above the sea*, which causes greater rarity of the air, and

* Comptes Rendus hebdomadaires des Séances de l'Académie des Sciences, June 30th, 1873.

consequently less abstraction of heat from the body. It also secures greater transcalency in the atmosphere by a position above the chief region of aqueous precipitation, and which is comparatively out of the reach of the dust and fuliginous matters that pollute the lower stratum of the air. On my journey from London to Davos I never saw the sun until I had arrived nearly at my destination; and during the greater portion of the fortnight of brilliant weather recorded above, there was a dull leaden sky at Zürich, about 60 miles distant.

2. *Thick and (during the winter months) permanent snow,* which reflects the solar heat and prevents the communication of warmth to the air, and consequently the production of atmospheric currents. In still, though cold, air the skin is well known to be less chilled than in much less cold air, which impinges with considerable velocity upon the surface of the body. The effect of motion through the air upon the sensation of warmth and cold at Davos is very striking. Sitting perfectly still in the sunshine, the heat in mid winter is sometimes almost unbearable; on rising and walking about briskly, a delicious feeling of coolness is experienced; but on driving in a sledge, the cold soon becomes painful to the unprotected face and hands.

3. *A sheltered position favourable for receiving both the direct and reflected solar rays.*—In this respect Davos-Dörfli, situated opposite to the entrance of the Dischma valley, has the advantage over Davos-Platz, two miles lower down the valley; for, in the latter village, the sun rises on the 21st December $1^h\ 9^m$ later, and sets about ten minutes earlier, than at Dörfli.

All these conditions contribute not only to a high sun-temperature during the winter months, but also to a comparatively uniform radiant heat from sunrise to sunset.

In conclusion I will only point to the general bearing which these observations have upon winter refuges for invalids. While the primary conditions to be secured in such places must ever be fine weather and a sheltered position, the next in importance is, undoubtedly, exposure all day long to reflected, as well as direct, solar radiation. To accomplish this, a southern aspect and a considerable expanse of water, or nearly level snow, are necessary; and it is important that the sanitarium should be considerably, and somewhat abruptly, elevated above the reflecting surface, so that it may receive, throughout the entire day, the uninterrupted reflection of the sun's rays. At the sea-side, for instance, only those houses which command such an uninterrupted view of the sea, ranging from S.E. to S.W., as shows the reflection of the sun throughout the entire day, enjoy the full advantages of the place. At, or near, the sea-level, however, it is impossible, owing to the suspended matters in the lower regions of the atmosphere, to enjoy any thing approaching to a uniform temperature from sunrise to sunset. For this purpose it is necessary to leave the grosser air of the plains behind, and to ascend

some 4000 or 5000 feet into the mountains, when, in these latitudes at least, the reflecting surface must necessarily be snow.

In the above remarks I have confined myself strictly to the physical aspect of the subject; but it is obvious that, in seeking an alpine sanitarium, the patient comes under new conditions of respiration, and breathes air comparatively free from zymotic matter—circumstances which are probably not without profound influence upon his health.

III. Addition to the Paper, "Volcanic Energy: an attempt to develop its true Origin and Cosmical Relations" *. By Robert Mallet, A.M., C.E., F.R.S., M.R.I.A., &c. Received April 3, 1874.

(Abstract.)

Referring to his original paper (Phil. Trans. 1873), the author remarks here that, upon the basis of the heat annually dissipated from our globe being equal to that evolved by the melting of 777 cubic miles of ice at zero to water at the same temperature, and of the experimental data contained in his paper, he had demonstrated, in terms of mean crushed rock, the annual supply of heat derivable from the transformation of the mechanical work of contraction available for volcanic energy, and had also estimated the proportion of that amount of heat necessary to support the annual vulcanicity now active on our globe; but, from the want of necessary data, he had refrained from making any calculation as to what amount in volume of the solid shell of our earth *must* be crushed annually, in order to admit of the shell following down after the more rapidly contracting nucleus. This calculation he now makes upon the basis of certain allowable suppositions, where the want of data requires such to be made, and for assumed thicknesses of solid shell of 100, 200, 400, and 800 miles respectively.

From the curve of total contraction (plate x. Phil. Trans. part i. 1873) obtained by his experiments on the contraction of slags, he has now deduced partial mean coefficients of contraction for a reduction in temperature of 1° Fahr., for intervals generally of about 500° for the entire scale, between a temperature somewhat exceeding that of the blast-furnace and that of the atmosphere, or 53° Fahr. And applying the higher of these coefficients to the data of his former paper, and to the suppositions of the present, he has obtained the absolute contraction in volume of the nuclei appertaining to the respective thicknesses of solid shell above stated. In order that the shell may follow down and remain in contact with the contracted nucleus, either its thickness must be in-

* Read June 20, 1872; Phil. Trans. for 1873, p. 147.

creased, its volume remaining constant, or the thickness being constant, a portion of the volume must be extruded. The former supposition is not admissible, as the epoch of mountain-building has apparently ceased; adopting the second, the author calculates the volume of matter that must be crushed and extruded from the shell in order that it may remain in contact with the nucleus. He tabulates these results for the four assumed thicknesses of shell, and shows that the amount of crushed and extruded rock necessary for the heat for the support of existing volcanic action is supplied by that extruded from the shell of between 600 and 800 miles thickness, and that the volume of material, heated or molten, annually blown out from all existing volcanic cones, as estimated in his former paper, could be supplied by the extruded matter from a shell of between 200 and 400 miles in thickness.

On data which seem tolerably reliable the author has further been enabled to calculate, as he believes for the first time, the actual amount of annual contraction of our globe, and to show that if that be assumed constant for the last 5000 years, it would amount to a little more than a reduction of about 3·5 inches on the earth's mean radius. This quantity, mighty as are the effects it produces as the efficient cause of volcanic action, is thus shown to be so small as to elude all direct astronomical observation, and, when viewed in reference to the increase of density due to refrigeration of the material of the shell, to be incapable of producing, during the last 2000 years, any sensible effect upon the length of the day. The author draws various other conclusions, showing the support given by the principal results of this entirely independent investigation to the verisimilitude of the views contained in his previous memoir.

Presents received, April 16, 1874.

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"On the Comparative Value of certain Geological Ages (or groups of Formations) considered as items of Geological Time." By A. C. Ramsay, LL.D., V.P.R.S. Received December 16, 1873*.

There are several methods by which attempts have been made to estimate the value of minor portions of geological time, one of which is founded on calculations of the probable age of deltas, deduced from estimates, more or less accurate, of the quantity of matter annually carried in suspension in rivers, in relation to the area occupied by, and the thickness of, any given delta, such as that of the Mississippi. But as none of these deltas are completed, and as it is unknown when, in the course of terrestrial changes, such completion may take place, no one can, as yet, successfully attempt to apply this kind of knowledge to the amount of time that was occupied in the formation of any of the ancient geological deltas, such, for instance, as that of the Purbeck and Wealden area.

Mr. James Croll has, with considerable success, attempted to measure that portion of geological time which relates to the last great Glacial epoch, founding his conclusions on astronomical data calculated backwards for a million of years; but, as yet, the precise beginning of that epoch has not, in my opinion, been shown; and in the absence of precise data respecting the number of local glacial episodes that may have preceded the last, and the complicated calculations that would be necessary to measure these intervals, even if all these episodes were known, no data are yet accessible for the application of Mr. Croll's method to the greater part of geological time.

There are other ways in which the subject has been approached, but always, of necessity, with a total want of definiteness with regard to their value in the measurement of time. The relative thickness of different formations gives no clue, or only a very slight one, to the solution of the question. Again, when in great and thick formations that spread over wide areas, such as those of Silurian age, an upper part of the series is found to lie quite unconformably on the lower half, it requires but little experience in geology to infer that the unconformity indicates a long lapse of unknown time, unrepresented by strata over a given area. When we link such phenomena of striking unconformity with the disappearance in that area of some of the genera and most of the species in the older strata, and their replacement by new and, to a great extent, generally of closely allied forms, this addition to our data gives no clear help in the absolute measurement of time; for no one as yet has even dared to speculate on the length of time that may have been necessary for the production of results so remarkable as those deduced from the theory of evolution.

I am well aware of much that may be said on the other side of this particular question, such as that the incoming life of the later epoch

* Read Jan. 29, 1874. See *antè*, p. 145.

may be merely the result of migration from some other area or areas, where it lived contemporaneously with the forms imbedded in the older strata; but this by no means gets rid of the question of time, with those who may believe in an hypothesis so uncertain, if it so happen that they also uphold the doctrine of evolution. Looked at in this light, it is obvious that the balance of probability is largely in favour of the greater proportion of the specific forms in a new formation being, in the common meaning of the word, of later date than those of an older formation, on which the newer strata lie unconformably.

Neither is the main question altered by the circumstance that a proportion of Palæozoic genera are, in some parts of the world, occasionally and unexpectedly found along with Mesozoic associates. The fact remains, that changes in life have been produced, during lapses of time, in specific and consequently in generic forms, and that such contrasts of specific, and often of generic, forms are always most striking where marked unconformities are found of a kind which prove that the lower strata had previously been much disturbed, and, as land, had suffered much denudation before being again submerged.

Seeing that speculations such as those enumerated, even when founded on well-established facts, afford but little help in the absolute measurement of geological time, it has occurred to me to look at the question from another point of view, and, in a broad manner, to attempt to estimate the *comparative value* of long and distinct portions of geological time, all of which are represented by important series of formations.

In two papers* I have attempted to show that the Old Red Sandstone, Permian, and New Red series were all deposited, not in an open sea, but in great inland lakes, fresh or salt; and this, taken in connexion with the wide-spreading terrestrial character of much of the Carboniferous series, showed that a great continental age prevailed over much of Europe and in some other regions, from the close of the Upper Silurian epoch to the close of the Trias. The object of the present memoir is to endeavour to show the *value* of the time occupied in the deposition of the formations alluded to above, when compared with the time occupied in the deposition of the Cambrian and Silurian rocks, and of the marine and freshwater strata which were deposited between the close of the Triassic epoch and the present day.

Partly for the same reasons that I consider the Old Red Sandstone to have been a lake formation, so I think it probable that the red and purple Cambrian rocks of Scotland, Shropshire, and Wales were also chiefly deposited in inland waters, occasionally alternating, as at St. David's, with marine interstratifications, generally marked by grey slaty fossiliferous shales, somewhat in the same manner that several bands con-

* Quarterly Journal Geol. Soc. 1871 (vol. xxvii. pp. 189–198 & 241–254), "On the Physical Relations of the New Red Marl, Rhætic Beds, and Lower Lias," and "On the Red Rocks of England of older date than the Trias."

taining marine fossils are interstratified among the freshwater strata of the Miocene rocks of Switzerland. The probability of these Cambrian strata being partly of lacustrine origin is increased by the occurrence of analogous beds beneath the Silurian strata of the Punjab. There, in what is known as the Salt Range, I am informed by Professor Oldham, are certain red marly and sandy strata believed to be the general equivalents of our purple Cambrian rocks. They contain several thick beds of rock-salt, such as could only have been deposited by supersaturation due to solar evaporation, in the manner that rock-salt seems to have been formed in the Keuper Marl.

If the red Cambrian beds of Britain were partly deposited in inland waters, then it appears likely that our Silurian formations, from the so-called Menevian and Lingula beds upwards, were all deposited under marine conditions between two continental epochs, the close of the first of which is indicated by the nature of the Cambrian rocks, and the beginning of the second by the passage of the Upper Ludlow beds into the base of the Old Red Sandstone.

The physical conditions and long duration of the second continental epoch have been described in my two memoirs on the Red Rocks of England *. The faunas of the Cambrian and Lingula-flag series (which pass conformably into each other), in the comparative paucity of species and their fragmentary character, seem partly to indicate occasional inland shallow seas, possibly comparable to the great inlet of the Bay of Fundy; and this scanty life probably gives but a poor idea of the fuller fauna of the period, hints of which we get from the equivalent formations of Sweden and Bohemia.

In the 'Geology of North Wales' (1866) I have shown that there is a gradual passage between the Cambrian rocks and that portion of the Lingula-flag series now sometimes called Menevian; and, for some years, I have held that the whole series of formations, from the lowest known Cambrian to the top of the Ludlow beds, may, in Britain, be most conveniently classed under three groups: Cambrian, Lingula, and Tremadoc slates form the lowest group, succeeded *unconformably* by the second group, consisting of the Llandeilo and Bala, or Caradoc, beds; above these we have the Llandovery, or May Hill, beds, overlaid by the Wenlock and Ludlow series, the Llandovery beds lying quite *unconformably* on any and all of the formations of older date, from the Cambrian to the Caradoc strata inclusive. With each unconformable break in stratigraphical succession there is a corresponding break in the succession of species, very few (about 2¼ per cent. out of 68 known species) passing from the Tremadoc slate into the Llandeilo beds, while

* Also in a lecture subsequently given at the Royal Institution, in which this piece of geological history was put into a more consecutive form, and the substance of which was published in full in the 'Contemporary Review' for July 1873, and (in Paris) in the 'Revue Scientifique' of 14th June.

from the Caradoc Sandstone only about 11 per cent. pass onward into the Upper Silurian strata. These phenomena indicate gaps in geological time unrepresented in the Silurian series of Britain by stratified deposits, and, therefore, also unrepresented by genera and species, that, did we know them, might serve to link together the life of the unconformable formations in a more graduated succession of forms. I recapitulate these opinions, which were in part originally given in my first Presidential Address to the Geological Society (1863), because they bear on the arguments that follow.

Like the Cambrian and Silurian rocks, the Devonian strata have also been classified in three divisions by palæontologists—Lower, Middle, and Upper. In Britain the Lower Devonian fauna is poor in numbers, while it is rich both on the continent of Europe and in North America. In England both the Middle and Upper Devonian fossils are plentiful enough. According to Mr. Etheridge, out of 74 English forms 25 per cent. pass from the Lower into the Middle division; while, out of 268 forms, 25 per cent. pass from the Middle into the Upper Devonian strata. No one has yet proved that these breaks in palæontological succession in the Devonian strata are accompanied by unconformable stratification; but the entire region has never been accurately mapped according to the detailed methods of modern work. However this may turn out, the vast thicknesses of these strata, characterized, like the great Silurian divisions, by three marine faunas, of which the species are mostly distinct, would seem to indicate that the time occupied in their deposition may be fairly compared with that occupied in the accumulation of the Silurian series.

I accept the view that the Old Red Sandstone, as a whole, is the general equivalent in time of the Devonian formations, and probably of a good deal more; for our Lower Devonian beds have no defined base, and, therefore, their precise relation to the British Upper Silurian rocks is unknown, whereas the Upper Ludlow rocks of Wales and its borders pass conformably, and somewhat gradually, into the Old Red Sandstone. If the Devonian rocks be the equivalent of the Old Red Sandstone, it follows that *the time occupied in the deposition of the latter may have been as long as that taken in the deposition of the Cambrian and Silurian series.* This position is greatly strengthened by the thorough specific, and in great part generic, differences in the fossils of the Upper Ludlow and those found in the marine Carboniferous series—differences that, to my mind, indicate a long lapse of time, represented by the deposition of the marine Devonian strata, during which time the Old Red Sandstone was being elsewhere deposited in the large lakes of an ancient continent. These palæontological comparisons seem to me to indicate the vast length of time necessary for the accumulation of these old lacustrine strata.

The next question to be considered is, what time the deposition of the Old Red Sandstone may have taken, when compared with the time

occupied in the deposition of certain members of the Mesozoic series. This may be attempted, partly on stratigraphical and partly on palæontological considerations.

The Lower Lias, at its junction with the Middle Lias, or Marlstone, passes gradually into that formation on the coast-cliffs of Yorkshire, where it is impossible to draw a boundary-line between them, either lithologically or palæontologically. Both contain beds of the same kind of ironstone; and the marly and somewhat sandy clays, through about twenty feet of strata, are similar in character, while a good proportion of the fossils in these passage-beds are common to both formations. Higher up, where the Marlstone becomes more sandy, a suite of fossils, to a great extent new, appears, due apparently to altered conditions of the sea-bottom: the water was shallower and nearer shore; and the topmost strata of sandstone often contain many stem-like bodies, sometimes two or three feet in length, lying on the surfaces of the beds in curved lines, the same stems sometimes bending and crossing each other in a manner that strongly reminds the observer of the broken stalks of Laminarian seaweeds lying on a sandy shore, within close reach of a Laminarian zone. Taking these things into account, there seems to be a much more intimate connexion between the Lower and Middle than there is between the sandy beds of the Middle Lias and the Upper Lias clays of Yorkshire, between which, though there is a perfect conformity, yet a sudden break in lithological character occurs, accompanied by a nearly complete change of fossil species. But the three divisions being conformable to each other, the diversities of fossil contents, more or less, seem to be owing to changes in the physical condition of the sea, caused, in the case of the Upper Lias shale, to sudden depression of the area, which resulted in the deposition of the muddy sediments of the Upper Lias in deeper water than that which received the uppermost sediments of the Marlstone. In the Midland Counties, however, the lithological break between the Middle and Upper Lias is not so sudden, and, in that region, there is a greater community of species.

In Yorkshire the strata immediately above the Lias are of mixed terrestrial, freshwater, and marine beds; but even there and in the middle of England, as shown by Dr. Wright, there is a certain community of fossils in the passage-beds that unite the Upper Lias to the Inferior Oolite. There is no perfect stratigraphical or palæontological break between them; and when we pass in succession through all the remaining members of the truly marine Oolitic series of Gloucestershire, Somersetshire, and Dorsetshire, no real unconformity anywhere exists. The same species of fossils, in greater or less degree, are apt to be common to two or more formations; for example, such community exists between the fossils of the Inferior Oolite and those of the Cornbrash, between those of the freestones of the Inferior and Great Oolites, of the Stonesfield and Collyweston slates, and between those of the Kimmeridge and Portland Oolites.

The change of life in the sea-bottoms was, so to say, partly local, and due more to minor accidental physical causes than to that larger kind of change that is marked by great disturbance of a lower set of strata, long-continued denudation, and the subsequent unconformable deposition of a newer set of beds upon them, thus clearly indicating a long lapse of time unrepresented by stratified deposits over a given area. I therefore infer that the whole of the Liassic and Oolitic series must be looked upon as presenting the various phases of one facies of marine life, belonging to one geological epoch, marked by boundaries below and above which depended on definite physical conditions over a large area. Such a state of things in this Mesozoic epoch is comparable to the changes in the fossil contents of the various subformations of the Cambrian and Lingula-flag series, of which the Tremadoc slates form an upper member; and, in my opinion, the comparison holds good even partly in the manner of their deposition, parts of both series having been locally deposited in waters not marine. On these grounds, therefore, the Jurassic formations, as a whole, may be compared with these early Palæozoic formations *in the length of time occupied for the deposition of each.*

If this inference be just, then, in like manner, they may be compared with the Lower Devonian strata—in England poor in fossils as far as is yet known, but rich on the continent of Europe and in North America; and this (assuming that the Devonian and Old Red Sandstone strata are equivalents) implies that *a lower portion of the Old Red Sandstone may have taken as long for its deposition as the whole of the time occupied in the deposition of the Liassic and Oolitic series.*

It is now generally allowed that the Wealden beds of England are the freshwater and estuarine equivalents of the Lower and Middle Neocomian strata of the Continent, which, in a palæontological sense, may be said, in some degree, to be related to the uppermost Jurassic strata, in so far that a certain proportion of the species of Mollusca are common to both, as shown by Forbes and Godwin-Austen; while, in our own country, from the Lower Greensand (Upper Neocomian) about 14 or 15 per cent. of the fossils pass on into the Upper Cretaceous strata. The same kind of proportion, but in less degree, is found in the relations of the Tremadoc to the Llandeilo and Bala series, and of the latter to the Upper Silurian formations, and also of the Lower to the Middle, and of the Middle to the Upper, Devonian strata. Those last named being representatives in time of parts of the Old Red Sandstone, it follows *that the whole of the time occupied in the deposition of the Old Red Sandstone may have been equal to the whole of the time occupied in the deposition of all the Jurassic, Purbeck, Wealden, and Cretaceous strata collectively.*

The next term of the continental era under review is the Carboniferous epoch, which, in its various conditions and numerous local subdivisions, may with considerable propriety be compared to the Eocene period. The deposits of both are locally of marine, estuarine, freshwater, and

terrestrial origin, and both are clearly connected with long special continental epochs.

Next come the various members of the Permian series, which, if my published conclusions are correct, were partly formed in great inland lakes, analogous to the Caspian Sea and other salt lakes of Central Asia at the present day. Having been deposited in lakes, these subformations may, in this one respect, be compared to the lacustrine strata of Miocene age; and if Gastaldi's conclusions with regard to part of the Italian Miocene beds, and my own opinions respecting part of the Permian strata, be correct, each series shows evidence of having included a glacial episode.

Later than the Permian comes the New Red, or Triassic, series, which, in this region, is not directly connected with the Permian strata, in so far that, where they occur in contact, the New Red Sandstone is generally unconformable to the Permian beds. In the threefold division of the New Red series in France and Germany, the marine beds of the Muschelkalk (unknown in England) may be compared to the Lower or Coralline Crag strata; and, though the Keuper Marls of Britain and of much of the Continent were evidently deposited in inland continental salt lakes, in the region of the Alps the St. Cassian and Hallstadt marine beds, being equivalent to the Keuper Marls, may in this respect be compared to the Red Crag series. No one is, I think, likely to consider that the marine strata of Triassic age took a shorter time in their deposition than the marine beds of the Crag; and, if we take the New Red Sandstone into account, the probability is, that the whole of the Triassic series occupied in their deposition a much longer time than that taken in the deposition of the Pliocene marine strata.

In my opinion, a great Tertiary continental phase began with the Eocene strata; and that continent having undergone many physical changes, has continued, down to the present day, with a certain amount of identity; and an analogous, though not strictly similar, state of things prevailed for an older continent, during the deposition of a large part of the formations treated of in this memoir.

If the method founded on the foregoing comparisons be of value, we then arrive at the general conclusion, *that the great local continental era, which began with the Old Red Sandstone and closed with the New Red Marl, is comparable, in point of geological time, to that occupied in the deposition of the whole of the Mesozoic, or Secondary, series, later than the New Red Marl, and of all the Cainozoic, or Tertiary, formations, and, indeed, of all the time that has elapsed since the beginning of the deposition of the Lias down to the present day.* To attempt to prove this theorem is the special object of this paper; and if I have been successful, the corollary must be deduced that the modern continental era which followed the oceanic submersion of a wide area, during which the greater part of the Chalk was being deposited, has been of much

shorter duration than the older continent mentioned above in italics; and which, to us, seems so ancient, when we think that the Alps and the Jura had then no more than a rudimentary existence.

There are other points that bear on the comparative value of different epochs of geological time. During the older local continental epoch there flourished four distinct floras, those of the Old Red Sandstone, Carboniferous, Permian, and Triassic series. Of these the first three, notwithstanding considerable generic and complete specific differences, may yet be said to be of one Palæozoic type. The Triassic flora, as far as it is known, is of a mixed character, with generic affinities, however, that unite it more closely to the Jurassic flora than to that of the Permian age. The whole series may therefore be considered as resolving itself into two types—the first extending from the Old Red Sandstone to the Permian times, and the second belonging to the Trias.

During the later period that elapsed, from the beginning of the deposition of the Lias down to the present day, we have also four distinct floras—the first of Jurassic type, embracing the little we know of the Neocomian flora; the second, Cretaceous, which, as regards the Upper Cretaceous strata of Aix-la-Chapelle and of Greenland, is to a great extent of modern type; third, an Eocene, and, fourth, a Miocene flora—the last three being closely allied, and the Miocene flora of Europe, in its great features, being nearly indistinguishable, except in species, from the kind of grouping incident to some of the modern floras of the northern hemisphere. The whole of this series may, therefore, in European regions, be also considered as resolving itself into two types—the first Jurassic, and the second extending from the later Cretaceous times to the present day. In this respect, the analogy to the floras of two types of the more ancient continent is obvious; and, in both epochs, this kind of grouping is clearly connected with the lapse of time, which, in my opinion, may for each be of approximately equivalent value.

The evidence derived from terrestrial Vertebrata is not quite so simple. In the Old Red Sandstone none are yet known. In the Carboniferous rocks all the known genera (fourteen in Britain) are Labyrinthodont Amphibia. The same is the case, though the known genera are fewer in number, with the Permian rocks, excepting two land-lizards of the genus *Proterosaurus*. Labyrinthodonts seem to decrease still more in the number of species in the Trias; but Crocodiles appear, together with seven named genera of land-lizards, two genera of Anomodontia (*Dicynodon* and *Rhynchosaurus*), three genera of Deinosauria, and two of Marsupial Mammalia. As far as we yet know, therefore, this ancient continental fauna pretty nearly resolves itself into two types; and, just as the Triassic type of flora passed into Jurassic times, so the Triassic land-fauna does the same. The oldest, or Palæozoic, type (Carboniferous and Permian) is essentially Labyrinthodontian, and the second, or Triassic is, characterized by the appearance of many true land-lizards and other terrestrial reptiles, together with marsupial mammals; and this typical

fauna, as regards genera, with the exception of Labyrinthodontia and the appearance of Pterosauria, is represented, pretty equally, through all the remaining members of the Mesozoic formations, from Jurassic to Cretaceous inclusive. After this comes the great Pachydermatous Mammalian Eocene fauna, and after that the Miocene fauna, which, in its main characters, is of modern type.

The general result is that, from Jurassic to Cretaceous times inclusive, there was a terrestrial fauna in these regions, chiefly Reptilian, Saurian, and Marsupial, and, in so-called Cainozoic or Tertiary times, chiefly Reptilian and Placental. In brief, the old continental epoch that lasted from the beginning of the Old Red Sandstone to the close of the Trias, locally embraces two typical land-faunas—one Carboniferous and Permian, and one Triassic; while the later epoch, from the beginning of the Lias to the present day, also locally contained two typical land-faunas, the latter of which is specially Placental. (See Table.)

I am aware that such inferences are always liable to be disturbed by later discoveries, and I therefore merely offer the above suggestions as being in accordance with present knowledge.

Another point remains. The earliest known marine faunas, those of the Cambrian, Lingula-flag, and Tremadoc beds, include many of the existing classes and orders of marine life, which are much more fully developed in the succeeding Llandeilo and Bala strata, such as Spongida, Annelida, Echinodermata, Crustacea, Polyzoa, Brachiopoda, Lamellibranchiata, Pteropoda, Nucleobranchiata, and Cephalopoda. This important fact was insisted on by Professor Huxley in his Anniversary Address to the Geological Society in 1862. The inference is obvious, that in this earliest known varied life* we find no evidence of its having lived near the beginning of the zoological series. In a broad sense, compared with what must have gone before, both biologically and physically, all the phenomena connected with this old period seem to my mind to be quite of a recent description; and the climates of seas and lands were of the very same kind as those that the world enjoys at the present day—one proof of which, in my opinion, is the existence of great glacial boulder beds in the Lower Silurian strata of Wigtonshire, west of Loch Ryan†.

This conclusion, not generally accepted, has since been confirmed by Professor Geikie and Mr. James Geikie, both with regard to the Wigtonshire strata and to the equivalent beds in Ayrshire. In the words of Darwin, when discussing the imperfection of the geological record of this history, "we possess the last volume alone, relating only to two or three countries;" and the reason why we know so little of pre-Cambrian faunas, and the physical characters of the more ancient formations as originally deposited, is, that, below the Cambrian, strata we get at once involved in a sort of chaos of metamorphic strata.

* Earliest known except the Huronian *Aspidella Terranovica* and the Laurentian *Eozoon Canadense*.

† See Philosophical Magazine, vol. xxix. p. 289, 1865.

The connexion of this question with the principal subject of this paper, that of *the comparative value of different geological eras as items of geological time*, is obvious. I feel that this subject is one of great difficulty; and, as far as I know, this is the first time that any attempt of the kind has been made to solve the problem. If my method be incorrect, it may yet help to suggest a better way to some one else; and in the meanwhile, even if partly heterodox, I hope it may deserve toleration.

Classification of Faunas (Terrestrial, Freshwater, and Estuarine) into Groups.

Formations.	Class.	Order.	Number of Genera.
Old Red Sandstone	......	......	No Vertebrata known except fish.
1 Carboniferous .	Amphibia...	Labyrinthodontia	11 E.
	,,	,,	3 C.
1 Permian......	,,	,,	3 E.
	,,	,,	1 C.
	Reptilia ...	Lacertilia.........	1 E.
2 Trias	Amphibia...	Labyrinthodontia	3 E.
	Reptilia ...	Crocodilia.........	1 E.
	,,	Lacertilia	4 E.
	,,	,,	3 C.
	,,	Anomodontia ...	1 E.
	,,	Deinosauria	2 E.
	,,	,,	1 C.
	Mammalia .	Marsupialia	2 E.
1 Jurassic	Reptilia ...	Chelonia	4 E.
	,,	Crocodilia	4 E.
	,,	,,	1 C.
	,,	Lacertilia	4 E.
	,,	Deinosauria......	2 E.
	,,	,,	1 C.
	,,	Pterosauria......	2 E.
	Mammalia .	Marsupialia	15 E.
	,,	,,	1 C.
1 Weald and Neocomian	Reptilia ...	Chelonia	4 E.
	,,	Crocodilia	8 E.
	,,	Deinosauria ...	9 E.
	,,	Pterosauria......	1 E.
1 Upper Cretaceous	,,	Crocodilia	3 E.
	,,	Pterosauria......	1 E.
	,,	Lacertilia	9 E.
2 Eocene.........	,,	Chelonia	4 E.
	,,	Crocodilia	3 E.
	,,	Ophidia	2 E.
	Mammalia .	Pachydermata...	11 E.
	,,	,,	Many more in France.
2 Miocene	,,	,,	No Vertebrata certain in England, but a large Mammalian fauna on the Continent, chiefly of modern type. The Pliocene and Post-Pliocene terrestrial faunas are of course still more modern.

The letter E. means English, C. Continental and not known in England; but as the physical phenomena connected with the Continental strata in which they are found are, in the main, identical with those that affect the English rocks, the European Continental

May 21, 1874.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, followed by Dr. SHARPEY, Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Structure and Development of *Peripatus capensis*." By H. N. MOSELEY, M.A., Naturalist to the 'Challenger' Expedition. Communicated by Prof. WYVILLE THOMSON, F.R.S. &c., Director of the Scientific Civilian Staff of the Expedition. Received April 9, 1874.

(Abstract.)

The author commences by expressing his obligations to Professor Thomson, who gave him assistance in some parts of his work, and every encouragement in the further prosecution of it.

Specimens of *Peripatus* were collected at the Cape of Good Hope during the stay of H.M.S. 'Challenger' at Simon's Bay, with a view to the investigation of the development of the animal. A specimen was dissected and at once seen to be provided with tracheæ, and to contain far developed young. This led to as careful an examination being made as time would permit, and hence the present paper. The most modern paper on *Peripatus* is that of Grube*. Grube, after examining the anatomy of the animal, came to the conclusion that it was hermaphrodite, and placed it among the "Bristle-Worms" in a separate order, Onychophora. Grube has been followed in most text-books, such as those of Claus and Schmarda; but uncertainty on the matter has been generally felt. De Quatrefages † follows Gervais in placing *Peripatus* in affinity with the Myriopods, and the result of the present investigation is to show that he is not far wrong.

The species made use of appears to be *Peripatus capensis*, described by Grube in the Zoological Series of the 'Novara' expedition. The animal has invariably seventeen pairs of ambulatory members, a pair of oral papillæ, and two pairs of horny hooked jaws, shut in by tumid lips. The specimens found varied in length from 1·6 to 7 centims., in the contracted condition. About thirty specimens were found, all of them but one at Wynberg, between Simon's Bay and Cape Town. The animals appear to be somewhat local and not very abundant; they live in damp places under trees, and especially frequent rotten willow-wood.

* Müller's Archiv, 1853. † Hist. des Annelés.

They feed on rotten wood. They are nocturnal in their habits. They coil themselves up spirally like *Iulus* when injured. They have a remarkable power of extension of the body, and when walking stretch to nearly twice the length they have when at rest. They can move with considerable rapidity. They walk with the body entirely supported on their feet. Their gait is not in the least like that of worms, but more like that of caterpillars. When irritated they shoot out with great suddenness from the oral papillæ a peculiarly viscid tenacious fluid, which forms a meshwork of fine threads, with viscid globules on them at intervals, the whole resembling a spider's web with the dew upon it. The fluid is ejected at any injuring body, and is probably used in defence against enemies, such as insects, which would be held powerless for some time if enveloped in its meshes. The fluid is not irritant when placed on the tongue, but slightly bitter and astringent; it is as sticky as birdlime: flies, when they light in it, are held fast at once. The fluid is structureless, but presents an appearance of fine fibrillation when dry. The animal is best obtained dead in an extended condition by drowning it in water, which operation takes four or five hours.

Only those points in anatomy are touched on which appear to have hitherto been wrongly or imperfectly described.

The intestinal tract is not straight, as described by Grube, but longer than the body, and usually presents one vertical fold; it presents numerous irregular sinuous lateral folds, but is not enlarged in every segment, as stated by Grube. Special regions, a muscular pharynx, short œsophagus, long stomach, and short rectum are distinguished in the tract. The viscid fluid ejected from the oral papillæ is secreted by a pair of ramified tubular glands lying at the sides of the stomach and stretching nearly the whole length of the body. These glands are those described by Grube as testes; they show a common glandular structure, but no trace of testicular matter. A pair of enlargements on the ducts of the glands, provided with spirally arranged muscles, serve as ejaculatory reservoirs. The lateral elongate bodies lying outside the nerve-cords, considered by Grube to be vessels, show a fatty structure, vary much in extent, and are probably to be regarded as representing the fatty bodies of Tracheata.

No structure like that of the heart of Myriopods was found in the dorsal vessel.

The tracheal system consists of long fine tracheal tubes, which very rarely branch: these arise, in densely packed bunches, from short common tubes, which open all over the body by small outlets in the epidermis; these outlets have no regular structure and are difficult to see. The whole of the tracheal system, very conspicuous in the fresh condition, becomes almost invisible when the animal examined has been a short time in spirit, and the air has been thus removed from the tracheæ. Hence the failure of Grube to see them. The tracheæ

are distributed in meshworks to all the viscera. The spiral filament is very imperfectly developed. A row of larger oval spiracles exists along the middle line of the under surface, the spiracles being placed opposite the interspaces of the feet, but not quite regularly. Other large spiracles exist on the inner sides of the bases of the feet. A large supply of tracheæ goes to the rectum and muscular pharnyx. In many points the structure of the tracheal system resembles that in *Iulus*.

Peripatus is not hermaphrodite. Out of thirty specimens about ten were males. No outward distinction of the sexes could be discovered. The female organs consist of a small oblong ovary situate behind the stomach, about one sixth of the length from the end of the body; from this lead a pair of oviducts, which, at their terminations, become enlarged and perform a uterine function, appearing, when filled with embryos, like a string of sausages. In nearly all cases, even when the embryos were far advanced, two large masses of spermatozoa were found in the ovary, and others attached to the ovisacs externally. A long loop, formed by the oviducts on each side being quite loose in the body, becomes often thrown into a knot through the constant protraction and retraction of the body-wall. The knot is known to sailors as an overhand knot on a bight. The knot sometimes becomes drawn very tight, and then prevents the passage of the embryos above it. A case was met with in which this had occurred. The upper parts of the oviducts were mortified off at the knot, and remained attached only to the ovary. The ducts were dilated into large single sacs, the usual constrictions between the embryos having disappeared, and were full of decomposed embryos and fatty tissue. The knot was met with in many specimens—in some cases on both sides of the body, in others (as in that figured) on only one. The oviducts unite in a short common tube to open at the simple vulva. The male organs consist of a pair of large ovoid testes, surmounted by short tubular prostates. The vasa deferentia are long and tortuous, forming, near the testes, spiral coils in which the ducts are enlarged, and which may be called vesiculæ seminales. A muscular ejaculatory tube, or penis, lies on one side of the body—sometimes on one, sometimes on the other. One vas deferens passes across, at the end of the body, under both nerve-cords to join the penis; the other takes a more direct course, not passing under the cords at all. In the original condition both ducts probably passed one under each nerve-cord, to join the centrally placed common terminal tube, homologous with that of the female organs.

The spermatozoa are filamentary, as in insects and in *Scolopendra*, but not in *Iulus*. Their development is described. They are very long, and their tails have a spiral movement as well as an undulatory one. They twist into all sorts of loops.

The muscular tissue of *Peripatus* is unstriated.

The development of *Peripatus* was only partially followed. As a rule,

all the embryos found in one mother are of the same age. In some cases slight differences were found, which were very valuable for determining the development of the parts of the mouth. The embryos lie coiled up in simple hyaline envelopes, enclosing an ovoid cavity, within the enlargements of the uterine tubes. In the earliest stage observed the embryo had large round cephalic lobes and was without members, but showed distinct segmentation about its middle; it was coiled up spirally, the head being free, the tail in the axis of the coil. Later on the embryo becomes bent round in an oval, with the tip of the tail resting between the antennæ.

The front members are formed first: they arise as undulations of the lateral wall of the body, which become pushed further and further outwards, and are at first hollow, formed of two layers of cells, the inner of which is reflected over the intestine. The members form one after another, from the head downwards. A line of segmentation is formed across the body before the pair of members swells out, but disappears as they develop. The wall of the digestive tract is, in the early condition, drawn out laterally at each interspace between the pairs of members, to become attached there to the body-wall. The cephalic lobes early show traces of a separation into two segments, anterior and posterior; from them, anteriorly, bud out the antennæ, which gradually become more and more jointed. The mouth forms before the anus.

The full number of body-members is very early attained. The second pair is the largest at first, but subsequently become the small oral papillæ. The first pair turn inwards towards the primitive mouth-opening, and, developing their claws greatly, form the pair of horny jaws; these are covered by processes which grow down from the lower part of the head, and which eventually unite with the tissues at the bases of the oral tentacles and form the tumid lips, which, eventually closing in, hide all the parts of the mouth in the adult. The head-processes are probably homologous with the mandibles of higher Tracheata, the horny jaws with the maxillæ and the oral papillæ with the foot-jaws of *Scolopendra*; a regular labrum is formed by a downward growth from the front of the head, but is eventually shut in by the tumid lips.

It is uncertain whether a corresponding structure beneath the mouth represents the second under lip of *Scolopendra* or a true labium. The foot-claws are developed in invaginations of the tips of the ambulacral members. The young members develop five joints each, the typical number in insects, and one which seems to be retained in the adult.

In the present state of our knowledge concerning the structure of *Peripatus*, the most remarkable fact in its structure is the wide divarication of the ventral nerve-cords. The fact was considered remarkable and dwelt upon in all accounts of *Peripatus* before the existence of tracheæ in the animal was known, and when it was thought

to be hermaphrodite, but it is doubly remarkable now. The fact shuts off at once all idea of *Peripatus* being a degenerate Myriopod, the evidence against which possibility is overwhelming. The bilateral symmetry and duplicity of the organs of the body, the absence of striation in the muscles, of periodical moults of the larval skin in development, and of any trace of a primitive three-legged condition, taken in conjunction with the divarication of the nerve-cords, are conclusive. The parts of the mouth are not to be regarded as degraded to any great degree; and homologies for some of them, at least, may perhaps be found amongst the higher Annelids. The structure of the skin is not at all unlike that in some worms, especially in its chitinous epidermic layer, which occasionally strips off in large pieces as a thin transparent pellicle. The many points of resemblance of *Peripatus* to Annelids need not be dwelt upon; they led to its former placing in classification; but it is difficult to understand how the very unannelid-like structure of the foot-claws did not lead others, beside De Quatrefages, to draw a line between *Peripatus* and the Annelids. In being unisexual, *Peripatus* is like the higher Annelids, as well as the whole of the higher Tracheata. To Insects *Peripatus* shows affinities in the form of the spermatozoa, and the elaboration, structure, and bilateral symmetry of the generative organs, though there is a very slight tendency towards the unilaterality of Myriopods in the male organs.

To Insects, again, it is allied by the five-jointing of the feet and oral papillæ and the form and number of its claws. It should be remembered that spiders' feet are two-clawed, as are those of some Tardigrades, and that some of these latter forms have two-clawed feet in the early condition even when they possess more claws in the adult state. In Newport's well-known figure of the young *Iulus* with three pairs of limbs, the tips of these latter are drawn with *two* hair-like claws; these are not mentioned in the text. To the ordinary lepidopterous larva the resemblances of *Peripatus* are striking—as, for example, the gait, the glands (so like in their function and position to silk-glands), the form of the intestine, and the less perfect concentration of the nervous organs, as in larval insects. To Myriopods *Peripatus* is allied by the great variety in number of segments in the various species, in its habits, and in these especially to *Iulus*. The parts of the mouth perhaps show a form out of which those of *Scolopendra* were derived by modification; but the resemblance may be superficial. Our knowledge is not yet sufficient to determine such points. The usual difficulties occur in the matter. Segments may have dropped out or fused, and their original condition may not be represented at all in the process of development. In structure *Peripatus* is more like *Scolopendra* than *Iulus*, viz. in the many joints to the antennæ (in Chilognaths never more than fourteen), in the form of spermatozoa, and in being viviparous, as are some *Scolopendræ*; further, in the position of the orifices of the generative glands and in

the less perfect concentration mesially of the nerve-cords in *Scolopendra.*

Peripatus thus shows affinities, in some points, to all the main branches of the family tree of Tracheata; but a gulf is fixed between it and them by the divarication of the nerve-cords: tending in the same direction are such facts as the non-striation of the muscles, the great power of extension of the body, the arrangement of the digestive tract in the early stage, the persistence of metamorphosis, and the nature of the parts of the mouth, the full history of the manner of origin of these being reserved.

There are many speculations as to the mode of origin of the tracheæ themselves in the Tracheata. Professer Häckel ('Biologische Studien,' p. 491) follows Gegenbaur, whose opinion is expressed in his 'Grundzüge der vergleichenden Anatomie,' p. 441. Gegenbaur concludes that tracheæ were developed from originally closed tracheal systems, through the intervention of the tracheal gills of primæval aquatic insects now represented as larvæ. If *Peripatus* be as ancient in origin as is here supposed, the condition of the tracheal system in it throws a very different light on the matter. *Peripatus* is the only Tracheate with tracheal stems opening diffusely all over the body. The Protracheata probably had their tracheæ thus diffused, and the separate small systems afterwards became concentrated along especial lines and formed into wide main branching trunks. In some forms the spiracular openings concentrated towards a more ventral line (*Iulus*); in others they took a more lateral position (Lepidopterous larvæ, &c.). A concentration along two lines of the body, ventral and lateral, has already commenced in *Peripatus.* The original Protracheate being supposed to have had numerous small tracheæ diffused all over its body, the question as to their mode of origin again presents itself. The peculiar form of the tracheal bundles in *Peripatus*, which consist of a number of fine tubes opening into the extremity of a single short common duct leading to the exterior of the body, seems to give a clue. The tracheæ are, very probably, modified cutaneous glands, the homologues of those so abundant all over the body in such forms as *Bipalium* or *Hirudo.* The pumping extension and contraction of the body may well have drawn a very little air, to begin with, into the mouths of the ducts; and this having been found beneficial by the ancestor of the Protracheate, further development is easy to imagine. The exact mode of development of the tracheæ in the present form must be carefully studied; there was no trace of these organs in the most perfect state of *Peripatus* which I obtained.

Professor Gegenbaur's opinion on the position of *Peripatus* ('Grundzüge der vergleichenden Anatomie,' p. 199) is, that its place among the worms is not certain, but that, at any rate, it connects ringed worms with Arthropods and flat worms. The general result of the present inquiry is to bear out Professor Gegenbaur's opinion; but it points to the connexion of the ringed and flat worms, by means of this inter-

mediate step, with three classes only of the Arthropods—the Myriopods, Spiders, and Insects, *i. e.* the Tracheata. From the primitive condition of the tracheæ in *Iulus*, and the many relations between *Peripatus* and *Scolopendra*, it would seem that the Myriopods may be most nearly allied to *Peripatus*, and form a distinct branch arising from it and not passing through Insects. The early three-legged stage may turn out as of not so much significance as supposed. If these speculations be correct, the Crustacea have a different origin from the Tracheata. *Peripatus* itself may well be placed amongst Professor Häckel's Protracheata; Grube's term Onychophora becomes no more significant than De Blainville's Malacopoda. Some notions of the actual history of the origin of *Peripatus* itself may be gathered from its development.

In conclusion I would beg indulgence for the many defects in this paper, due to the hurry with which it was written (all available time, almost up to the last moment of our sailing for the Antarctic regions, having been consumed in actual examination of the structure of *Peripatus*), and due, further, to the impossibility of referring to original papers in any scientific library. At all events it is hoped that *Peripatus* has been shown to be of very great zoological interest, as lying near one of the main stems of the great zoological family tree, and that further examination of the most minute character into the structure of this animal will be well repaid.

H.M.S. 'Challenger,' Simon's Bay, Cape of Good Hope,
December 17, 1873.

II. "The Uniform Wave of Oscillation." By John Imray, M.A., Memb.Inst.C.E. Communicated by W. Froude, M.A., F.R.S. Received April 11, 1874.

(Abstract.)

The results of the investigation worked out in this paper correspond with those previously deduced by other analysts, particularly by Mr. W. Froude, F.R.S. The paper is therefore presented, not because it discloses any novel result, but rather as an example of a method which the author has found useful in the discussion of other dynamical problems.

The object of the paper is to trace the conditions under which the separate molecules of a liquid such as water move, when a body of that liquid is in a state of oscillatory wave-movement. It is assumed that the wave-movement is established in a channel of uniform width and of length and depth so great that the conditions of motion are not affected by the interference of fixed ends or a fixed bottom.

The wave treated of has as its characteristics permanence of form and uniformity of apparent velocity.

In order that these conditions may be fulfilled, it is assumed, as being

capable of ready geometrical demonstration, that all molecules which in repose would be at the same level, move in equal and similar trajectories, but that each molecule towards the one hand, as towards the right, is by a certain interval of time in advance of the contiguous molecule on the left. It is also taken as a necessary condition of the wave-movement that the excursions of the molecules are periodic, and effected in closed orbits returning into themselves.

With these general postulates, the author proceeds to investigate first the conditions necessary to maintain continuity of the liquid, or the constancy of the vertical sectional area of an elementary portion of the liquid, in all parts of its orbit. He then traces the operation on such an element of the forces to which it is subjected, these forces being gravity, or the weight of the element itself, and the pressure directed on it by the surrounding liquid.

The liquid in repose is supposed to be divided into numerous horizontal strata, each stratum forming an undulating film when the wave-movement is established. The length of any such stratum is supposed to be divided into numerous portions, the width of each of which is the distance apparently traversed by the wave in a very short interval of time. By taking the depth of a stratum, and the interval of time which determines the width of one of its divisions, such that the element of liquid may be considered a parallelogram of constant area, the several differential equations expressing the continuity of the liquid and the effect of the forces on the element are developed in an integrable form.

The parallelogram representing the liquid element is determined in its form and position by the position of the points at its four angles.

One of those points, namely that at the lower left-hand angle, is assumed to move in a path the horizontal and vertical coordinates of which, x and y, are referred to an origin situated at a height h measured from the bottom of the liquid, and the position of the point in its path is taken at a time t reckoned from the epoch when the point was vertically under its origin. The point at the lower right-hand angle of the parallelogram is referred to an origin on the same level with the former, but separated horizontally from it by a space, $v\Delta t$, where v is the apparent velocity of the wave, and Δt is the short interval of time by which the one point is in advance of the other in its trajectory. The coordinates x and y of the first point being functions of h and t, those of the other point are the same functions of h and $t+\Delta t$. The upper left-hand point being referred to an origin which is at a height Δh above the level of the former origins, but being taken as contemporaneous in its movement with the point below it, its coordinates are functions of $h+\Delta h$ and t; and in like manner the coordinates of the upper right-hand point are functions of $h+\Delta h$ and $t+\Delta t$.

As it does not *à priori* appear that the origin of the upper point must be vertically above that of the lower (though in the course of the inves-

tigation it is shown that this must be the case), the author has, in the first instance, assumed that the upper origin is somewhat in advance of the lower, the amount of such advance being a quantity of the order Δh, which he has taken as $m\Delta h$ (it being afterwards proved that $m=0$).

With this nomenclature, the equation of continuity is deduced in the following terms:—

$$\left(v \pm \frac{dx}{dt}\right)\left(1+\frac{dy}{dh}\right)-\left(m+\frac{dx}{dh}\right)\frac{dy}{dt}=A,$$

a constant area independent of t.

The pressure p at the lower left-hand angle of the element being a function of h and t, equations are deduced giving values for the horizontal accelerating force, $\frac{d^2x}{dt^2}$, and the vertical accelerating force including gravity, $g+\frac{d^2y}{dt^2}$, in terms of the differential coefficients of x, y, and p.

From these equations it is shown that $\frac{dp}{dt}=0$, or that the pressure along any wave-stratum is uniform; and this result leads to the simplification of the differential equations.

From the integration of those equations it is shown that every molecule of the liquid revolves with uniform velocity, and with the same angular velocity at all depths, in a truly circular orbit, the radius of which depends on the depth of the molecule below the surface of the liquid. The law of variation of the radius is, that while the depths increase in arithmetical progression, the radius diminishes in geometrical progression, or that the logarithm of the reciprocal of the radius is directly proportional to the depth of the centre.

The resultant of the forces acting on a molecule is shown to be always normal to the profile of the wave-surface of which the molecule forms a part, such resultant being compounded of gravity, a constant force acting vertically downwards, and of the centrifugal force of the molecule, also a constant force acting radially outwards from the centre of the orbit. The direction and magnitude of this resultant are represented by the position and length of a line drawn from any point in the orbit to a fixed point in the vertical line passing through the centre of the orbit. The liquid element in traversing its circular path varies in width and in height to suit the varying direction of the forces acting on it, its greater height giving a greater hydrostatic pressure at the upper part of the orbit, where the centrifugal force is opposed to gravity, and its less height giving a less hydrostatic pressure at the lower part of the orbit, where the centrifugal force acts along with gravity. Thus the uniformity of pressure throughout the orbit is maintained.

As a molecule revolves uniformly round the centre of its orbit, this centre is the mean centre of gravity of the molecule during a complete

period. It is shown that during wave-movement this centre is elevated above the level that would be occupied by the molecule in repose, a height due to the *vis viva* of the molecule.

The profile of any wave-stratum is a trochoid, the length of which is the distance from hollow to hollow or from crest to crest, and the height is the diameter of the orbit of the molecule belonging to that stratum. The highest possible wave is that where the trochoid becomes the cycloid, or where the length of the wave is equal to the circumference of the orbit. No trochoid of greater height is physically possible, as such a curve must have a looped crest, where the liquid molecules would have to cross the paths of each other, producing broken water.

The velocity and period of a wave, and the angular and actual velocities of the liquid molecules, are deduced in terms of the length of the wave.

The general results of the investigation are shown by the following formulæ, in which the symbols employed are :—

L = length of wave from crest to crest.

v = velocity, or distance apparently traversed by the wave in a given unit of time.

T = the period, or time occupied by the passage of the whole wave.

g = gravity (32 feet per second).

R = radius of the orbit of a molecule at

H = height measured from bottom, and

r = radius at

h = height.

x = horizontal, and

y = vertical ordinate of molecule in stratum at height h and at time t, from the epoch when the molecule is at its lowest point, or when $x=0$.

Then

$$r=Re^{\frac{g}{v^2}(h-H)},$$

$$\left.\begin{aligned} x&=r\sin\frac{g}{v}t,\\ y&=-r\cos\frac{g}{v}t, \end{aligned}\right\}\text{origin being the centre of orbit,}$$

$$v=\sqrt{\frac{gL}{2\pi}},$$

$$T=\sqrt{\frac{2\pi L}{g}},$$

$$\frac{g}{v}=\text{angular velocity}=\sqrt{\frac{2\pi g}{L}}.$$

III. "On Combinations of Colour by means of Polarized Light." By W. Spottiswoode, M.A., Treas. & V.P.R.S. Received April 8, 1874.

The results of combining two or more colours of the spectrum have been studied by Helmholtz, Clerk Maxwell, Lord Rayleigh, and others; and the combinations have been effected sometimes by causing two spectra at right angles to one another to overlap, and sometimes by bringing images of various parts of a spectrum simultaneously upon the retina. Latterly also W. v. Bezold has successfully applied the method of binocular combination to the same problem (Poggendorff, Jubelband, p. 585). Some effects, approximating more or less to these, may be produced by chromatic polarization.

Complementary Colours.—First as regards complementary colours. If we use a Nicol's prism, N, as polarized, a plate of quartz, Q, cut perpendicularly to the axis, and a double-image prism, P, as analyzer, we shall, as is well known, obtain two images whose colours are complementary. If we analyze these images with a prism, we shall find, when the quartz is of suitable thickness, that each spectrum contains a dark band, indicating the extinction of a certain narrow portion of its length; these bands will simultaneously shift their position when the Nicol N is turned round. Now, since the colours remaining in each spectrum are complementary to those in the other, and the portion of the spectrum extinguished in each is complementary to that which remains, it follows that the portion extinguished in one spectrum is complementary to that extinguished in the other; and in order to determine what portion of the spectrum is complementary, the portion suppressed by a band in any position we please, we have only to turn the Nicol N until the band in one spectrum occupies the position in question, and then to observe the position of the band in the other spectrum. The combinations considered in former experiments are those of simple colours; the present combinations are those of mixed tints, viz. of the parts of the spectrum suppressed in the bands. But the mixture consists of a prevailing colour, corresponding to the centre of the band, together with a slight admixture of the spectral colours immediately adjacent to it on each side.

The following results, given by Helmholtz, may be approximately verified:—

Complementary Colours.	
Red,	Green-blue;
Orange,	Cyanic blue;
Yellow,	Indigo-blue.
Yellow-green,	Violet.

When in one spectrum the band enters the green, in the other a band will be seen on the outer margin of the red and a second at the opposite

end of the violet—showing that to the green there does not correspond one complementary colour, but a mixture of violet and red, *i. e.* a reddish purple.

Combination of two Colours.—Next as to the combination of two parts of the spectrum, or of the tints which represent those parts. If, in addition to the apparatus described above, we use a second quartz plate, Q, and a second double-image prism, P_1, we shall form four images, say O O, O E, E O, E E; and if A, A′ be the complementary tints extinguished by the first combination Q P alone, and B, B′ those extinguished by the second $Q_1 P_1$ alone, then it will be found that the following pairs of tints are extinguished in the various images:—

Image.	Tints extinguished.
O O	B, A,
O E	B′, A′,
E O	B′, A,
E E	B, A′.

It is to be noticed that in the image O E the combination $Q_1 P_1$ has extinguished the tint B′ instead of B, because the vibrations in the image E were perpendicular to those in the image O formed by the combination Q P. A similar remark applies to the image E E.

The total number of tints which can be produced by this double combination Q P, $Q_1 P_1$ is as follows:—

	4 single images,
	6 overlaps of two,
	4 overlaps of three,
	1 overlap of four.
Total..	15

Collateral Combinations.—The tints extinguished in the overlap O O+E O will be B, A, B′, A; but since B and B′ are complementary, their suppression will not affect the resulting tint except as to intensity, and the overlap will be effectively deprived of A alone; in other words, it will be of the same tint as the image O would be if the combination $Q_1 P_1$ were removed. Similarly the overlap O E+E E will be deprived effectually of A′ alone; in other words, it will be of the same tint as E, if $Q_1 P_1$ were removed. If therefore the Nicol N be turned round, these two overlaps will behave in respect of colour exactly as did the images O and E when Q P was alone used. We may, in fact, form a Table thus:—

Image.	Colours extinguished.
O O+E O	$B+A+B'+A=B+B'+A=A$
O E+E E	$B'+A'+B+A'=B+B'+A'=A'$.

And since the tints B, B′ have disappeared from each of these formulæ, it follows that the second analyzer P may be turned round in any direction without altering the tints of the overlaps in question.

In like manner we may form the Table

$$\begin{array}{ll} \mathrm{O\,O+E\,E} & B+A+B+A'=B+A+A'=B \\ \mathrm{O\,E+E\,O} & B'+A'+B'+A=B'+A+A'=B'. \end{array}$$

Hence if the Nicol N be turned round, these overlaps will retain their tints; while if the analyzer P_1 be turned, their tints will vary, although always remaining complementary to one another.

There remains the other pair of overlaps, viz.:—

$$\begin{array}{ll} \mathrm{O\,O+O\,E} & B+A+B'+A' \\ \mathrm{E\,O+E\,E} & B'+A+B+A'. \end{array}$$

Each of these is deprived of the pair of complementaries A, A′, B, B′; and therefore each, as it would seem, ought to appear white of low illumination, *i. e.* grey. This effect, however, is partially masked by the fact that the dark bands are not sharply defined like the Fraunhofer lines, but have a core of minimum or zero illumination, and are shaded off gradually on either side until at a short distance from the core the colours appear in their full intensity. Suppose, for instance, that B′ and A′ were bright tints, the tint resulting from their suppression would be bright; on the other hand, the complementary tints A and B would be generally dim, and the image $B+A$ bright, and the overlap $B+A+B'+A'$ would have as its predominating tint that of $B+A$; and similarly in other cases.

There are two cases worth remarking in detail, viz., first, that in which

$$B=A',\ B'=A,$$

i. e. when the same tints are extinguished by the combination Q P and by $Q_1 P_1$. This may be verified by either using two similar quartz plates $Q_1 Q_1$, or by so turning the prism P_1 that the combination $Q_1 P_1$ used alone shall give the same complementary tints as Q P when used alone. In this case the images have for their formulæ the following:—

$$\begin{array}{cccc} \mathrm{O\,O} & \mathrm{O\,E\,O} & \mathrm{E\,O} & \mathrm{E\,E} \\ A+A' & A+A' & 2A & 2A'; \end{array}$$

in other words, O O and E O will show similar tints, and E O, E E complementary. A similar result will ensue if $B=A$, $B'=A'$.

Again, even when neither of the foregoing conditions are fulfilled, we may still, owing to the breadth of the interference-bands, have such an effect produced that sensibly to the eye

$$B+A=B'+A';$$

and in that case

$$\begin{aligned} B'+A&=B+A-A'+A \\ &=B+A'+2A-2A', \end{aligned}$$

which imply that the images O O and O E may have the same tint, but that E O and E E need not on that account be complementary. They will differ in tint in this, that E E, having lost the same tints as E O, will have lost also the tint A, and will have received besides the addition of two measures of the tint A′.

Effect of Combinations of two Colours.—A similar train of reasoning might be applied to the triple overlaps. But the main interest of these parts of the figure consists in this, that each of the triple overlaps is complementary to the fourth single image, since the recombination of all four must reproduce white light: hence the tint of each triple overlap is the same to the eye as the mixture of the two tints suppressed in the remaining image; and since by suitably turning the Nicol N or the prism P_1, or both, we can give any required position to the two bands of extinction, we have the means of exhibiting to the eye the result of the mixture of the tints due to any two bands at pleasure.

Effect of Combinations of three Colours.—A further step may be made in the combination of colours by using a third quartz, Q_2, and a third double-image prism, P_2, which will give rise to eight images; and if C C′ be the complementaries extinguished by the combination $Q_2 P_2$, the formulæ for the eight images may be thus written:—

O O O	$C + B + A.$
O O E	$C + B' + A'.$
O E O	$C' + B' + A.$
O E E	$C' + B + A'.$
E O O	$C' + B + A.$
E O E	$C' + B' + A'.$
E E O	$C + B' + A.$
E E E	$C + B + A'.$

The total number of combinations of tint given by the compartments of the complete figure will be:—

$\frac{8}{1}$	=	8 single images.		
$\frac{8.7}{1.2}$	=	28 overlaps of two.		
$\frac{8.7.6}{1.2.3}$	=	56	"	three.
$\frac{8.7.6.5}{1.2.3.4}$	=	70	"	four.
$\frac{8.7.6}{1.2.3}$	=	56	"	five.
$\frac{8.7}{1.2}$	=	28	"	six.
$\frac{8}{1}$	=	8	"	seven.
1	=	1	"	eight.
	Total	255		

The most interesting features of the figure consist in this, that the subjoined pairs are complementary to one another, viz.:—

OOO	EOE
$C + B + A$	$C' + B' + A'$
EOO	OOE
$C' + B + A$	$C + B' + A'$
EEO	OEE
$C + B' + A$	$C' + B + A'$
EEE	OEO
$C + B + A'$	$C' + B' + A$

And if the prisms P, P_1, P_2 are so arranged that the separations due to them respectively are directed parallel to the sides of an equilateral triangle, the images will be disposed thus:—

	OEO	OOO	
EEO	EOO	OEE	OOE
	EEE	EOE	

The complementary pairs can then be read off, two horizontally and two vertically, by taking alternate pairs, one in each of the two vertical, and two in the one horizontal row; and each image will then represent the mixture of the three tints suppressed in the complementary image.

Low-tint Colours.—A slight modification of the arrangement above described furnishes an illustration of the conclusions stated by Helmholtz, viz. that the low-tint colours (couleurs degradées), such as russet, brown, olive-green, peacock-blue, &c., are the result of relatively low illumination. He mentioned that he obtained these effects by diminishing the intensity of the light in the colours to be examined, and by, at the same time, maintaining a brilliantly illuminated patch in an adjoining part of the field of view. If therefore we use the combination N, Q, P, P_1 (*i. e.* if we remove the second quartz plate), we can, by turning the prism P round, diminish to any required extent the intensity of the light in one pair of the complementary images, and at the same time increase that in the other pair. This is equivalent to the conditions of Helmholtz's experiments; and the tints in question will be found to be produced.

IV. "Further Experiments on the Transmission of Sound." By JOHN TYNDALL, D.C.L., LL.D., Professor of Natural Philosophy in the Royal Institution. Received May 21, 1874.

The author describes a number of experiments made with heterogeneous atmospheres obtained by saturating alternate layers of air with the vapours of various volatile liquids. Starting from his observation on the transmission of sound through a snow-storm on the Mer de Glace, in the winter of 1859, he shows the extraordinary power of sound to pass through the interstices of solid bodies as long as the continuity of the air is preserved. Sound, for example, penetrates through twelve layers of a silk handkerchief, while a single layer of the same handkerchief dipped into water, so as to fill the interstices, cuts off the sound.

He also describes numerous experiments with artificial fogs of a density so great that a depth of three feet sufficed to intercept the concentrated beam of the electric light; the effect of such fogs on sound was sensibly *nil*. Experiments were also executed on the illumination of such fogs by sudden flashes, obtained by the combustion of gunpowder or gun-cotton, or by the alternate extinction and revival of the electric and other lights. Such flashes promise to be extremely useful as fog-signals.

The author corrects the mistake of supposing that, in the experiments at the South Foreland, the lower trumpets were not compared with the higher ones. This, in fact, was the first step of the inquiry.

He also communicated an extraordinary instance of the interception of sound during one of the battles of the late American war.

In these experiments the author has been ably aided by his assistant, Mr. John Cottrell. An account of the experiments will be found in a paper now printing for the Philosophical Transactions.

V. "On some recent Experiments with a Fireman's Respirator." By JOHN TYNDALL, D.C.L., LL.D., Professor of Natural Philosophy in the Royal Institution. Received May 21, 1874.

In vol. clx. of the 'Philosophical Transactions,' 1870, p. 337, I refer to certain experiments on the "floating matter of the air," which were afterwards considerably expanded and in part described in my 'Fragments of Science.' These experiments, in which my object was to obtain optically pure air by filtration through cotton-wool, suggested to me the notion of a fireman's respirator. Cotton-wool had been previously employed by Schroeder and Pasteur in their experiments on spontaneous generation.

I had heard that smoke was a formidable obstacle to the fireman, and that cases of suffocation were not rare; hence the desire to construct a

respirator. My first trials were made with cotton-wool alone. Associated with the respirator was a mouthpiece with two valves: through one the inhaled air reached the lungs, having first passed through the cotton-wool, while through the other the exhaled air was discharged directly into the atmosphere. The smoke was generated in small rooms, and in some experiments in a cupboard; but though the irritation of the smoke was greatly mitigated by the cotton-wool, it was unbearable for any considerable time.

The cotton-wool was next carefully moistened with glycerine, no clots which could intercept the air being permitted. The respirator was distinctly improved by the stickiness of the fibres of the wool: still, when the smoke was very dense, an amount of irritation continued, which materially interfered with the usefulness of the respirator. Thinking it certain that the mechanically suspended matter would be intercepted by the moistened wool, I concluded that this residual irritation was due to the vaporous hydrocarbons generated during combustion: hence the thought of associating with the cotton-wool Dr. Stenhouse's excellent device of a charcoal respirator. The experiment was successful. With this combination it was possible to remain with comparative comfort for half an hour, or even an hour, in atmospheres a single inhalation of which without the respirator would be intolerably painful.

Captain Shaw, of the Metropolitan Fire Brigade, has worked energetically towards the completion of the respirator by associating with it a smoke-cap. Mr. Sinclair has done the same, and he informs me that the respirator is now in considerable demand.

Having heard from Captain Shaw that, in some recent very trying experiments, he had obtained the best effects from dry cotton-wool, and thinking that I could not have been mistaken in my first results, which proved the dry so much inferior to the moistened wool and its associated charcoal, I proposed to Captain Shaw to bring the matter to a test at his workshops in the city. He was good enough to accept my proposal, and thither I went on the 7th of May. The smoke was generated in a confined space from wet straw, and it was certainly very diabolical. At this season of the year I am usually somewhat shorn of vigour, and therefore not in the best condition for severe experiments; still I wished to test the matter in my own person. With a respirator which had been in use some days previously, and which was not carefully packed, I followed a fireman into the smoke, he being provided with a dry-wool respirator. I was compelled to quit the place in about three minutes, while the fireman remained there for six or seven minutes.

I then tried his respirator upon myself, and found that with it I could not remain more than a minute in the smoke; in fact the first inhalation provoked coughing.

Thinking that Captain Shaw himself might have lungs more like mine than those of his fireman, I proposed that he and I should try the

respirators; but he informed me that his lungs were very strong. He was, however, good enough to accede to my request. Packing the respirator with greater care, I entered the den with Captain Shaw. I could hear him breathe long, slow inhalations; and after the lapse of seven minutes I heard him cough. In seven and a half minutes he had to quit the place, thus proving that his lungs were able to endure the irritation seven times as long as mine could bear it. I continued in the smoke with hardly any discomfort for sixteen minutes, and certainly could have remained in it much longer.

During this time I was in a condition to render very material assistance to a person in danger of suffocation.

The smoke-cap I wore was one made by Mr. Sinclair, which has a mouthpiece similar to that used in the inhalation of nitrous oxide. But, to show the care necessary in packing the respirator, it is only necessary to remark that, with the packing furnished to me by Mr. Sinclair, it was not possible for either myself or Mr. Cottrell to continue in a dense smoke for more than three minutes; and even these were minutes of laborious breathing. Flannel disks are employed in these respirators, but I cannot recommend them. Cotton-wool carefully moistened and teased is, in my opinion, much better.

It is always possible to associate fragments of lime with the respirator, thus, if necessary, intercepting a portion of the carbonic acid. But in most fires we have a more or less free circulation of air; and I venture to think that not in one case in a thousand of actual fires would the combination of smoke and carbonic acid be so noisome as it was in the experiments here described.

The Society then adjourned over the Whitsuntide Recess, to Thursday, June 11.

June 4, 1874.

The Annual Meeting for the election of Fellows was held this day.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Sir James Alderson and General Boileau were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society:—

2 F 2

Isaac Lowthian Bell, F.C.S.
W. T. Blanford, F.G.S.
Henry Bowman Brady, F.L.S.
Thomas Lauder Brunton, M.D., Sc.D.
Prof. W. Kingdon Clifford, M.A.
Augustus Wollaston Franks, M.A.
Prof. Olaus Henrici, Ph.D.
Prescott G. Hewett, F.R.C.S.
John Eliot Howard, F.L.S.
Sir Henry Sumner Maine, LL.D.
Edmund James Mills, D.Sc.
Rev. Stephen Joseph Perry, F.R.A.S.
Henry Wyldbore Rumsey, M.D.
Alfred R. C. Selwyn, F.G.S.
Charles William Wilson, Major R.E.

Thanks were given to the Scrutators.

June 11, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

Mr. William Thomas Blanford, Dr. Thomas Lauder Brunton, Professor W. Kingdon Clifford, Mr. Prescott G. Hewett, Mr. John Eliot Howard, Dr. Edmund James Mills, the Rev. Stephen Joseph Perry, and Major Charles William Wilson were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Note on the Absorption-Spectra of Potassium and Sodium at low Temperatures." By H. E. Roscoe, F.R.S., and Arthur Schuster, Ph.D. Received April 30, 1874.

In order to obtain the absorption-spectrum afforded by the well-known green-coloured potassium vapour, pieces of the clean dry metal were sealed up in glass tubes filled with hydrogen, and one of these was then placed in front of the slit of a large Steinheil's spectroscope furnished with two prisms having refracting angles of 45° and 60°. The magnifying-power of the telescope was 40, and was sufficient clearly to separate the D lines with one prism. A continuous spectrum from a lime-light was used, and that portion of a tube containing the bright metallic globule of potassium was gently heated until the green vapour made its appearance. A complicated absorption-spectrum was then seen, a set of bands (α) in the red coming out first; whilst after a few moments two other groups appeared on either side of the D lines, the group β (less refrangible) being not so dark as the group γ. These bands are all shaded off towards the red, and in general appearance resemble those of the iodine-spectrum. In order to assure ourselves that the bands are not caused by the presence of a trace of an oxide, tubes were prepared in

which the metal was melted in hydrogen several times on successive days until no further change in the bright character of the globule could be perceived. On vaporizing the metal, which had been melted down to a clean portion of the tube, the bands were seen as before, and came out even more clearly, the globule, after heating, exhibiting a bright metallic surface. An analysis of the potassium used showed that it did not contain more than 0·8 per cent. of sodium, although, of course, the double line D was always plainly seen.

In order to ascertain whether an alteration in the absorption-spectrum of the metal takes place at a red heat, fragments of potassium were placed in a red-hot iron tube, through which a rapid current of pure hydrogen gas was passed, the ends of the tube being closed by glass plates. The magnificent green colour of the vapour was clearly seen at this temperature, on looking through the tube at a lime-light placed at the other end. Owing, doubtless, to the greater thickness or increased pressure of the vapour, the bands seen by the previous method could not be resolved by the small spectroscope employed, the whole of the red being absorbed, whilst a broad absorption-band in the greenish yellow was seen occupying the place of the group γ.

The positions of the bands obtained by the first method were measured by means of a telescope and distant scale, and the wave-lengths obtained by an interpolation curve, for which well known air-lines were taken as references. The following numbers give the wave-lengths of the most distinct, that is, the most refrangible edge of each band. As the measurements had to be made quickly, owing to the rapid darkening of the glass by the action of the metallic vapour, these numbers do not lay claim to very great accuracy, but fairly represent the relative positions of the band, and show that they do not always occur at regular intervals, although they are pretty regularly spread over the field and all are shaded alike.

Bands of potassium shaded off towards red. Wave-lengths in tenth-metres :—

6844		6459		6311		5949		5763	
6762		6430		6300	α	5930	β	5745	
6710		6400		6275		5901		5732	
6666		6379		6059		5860		5712	
6615	α	6357	α	6033		5842		5700	γ
6572		6350		6012	β	5821	γ	5690	
6534		6331		5988		5802		5674	
6494		6322		5964		5781		5667	

The bright potassium-lines in the red and violet were not seen reversed, the intensity of the lime-light being too small at both extremes to render an observation possible.

In order to ascertain whether the vapour of sodium, which, when seen in thin layers, appears nearly colourless, exhibits similar absorption-bands,

tubes containing the pure metal, which had been manufactured and preserved out of contact with any hydrocarbon, were prepared, the metal being obtained free from oxide and the absorption-spectrum being observed in the manner already described. As soon as the metal began to boil, a series of bands in the blue (Na γ) made their appearance, and shortly afterwards bands in the red and yellow (Na α), stretching as far as the D lines, came out. At this period of the experiment the D lines widened, thus blotting out a series of fine bands occurring in the orange (Na β), some of which, consequenly, could not be mapped. All the bands of the sodium-spectrum shade off, like the potassium-bands, towards the red.

When the vapour of sodium is examined in a red-hot iron tube, the colour of the lime-light, as seen through it, is a dark blue. As the sodium is swept away by the current of hydrogen passing through, the colour becomes lighter, and the transmitted rays can be analyzed by the spectroscope. At first, the whole red and green and part of the blue is cut out entirely. The D lines are considerably widened, and an absorption-band is seen in the green, apparently coinciding with the double sodium-line, which comes next in strength to the D lines. All the colours, therefore, seem to be shut out, except part of the orange, part of the green, and the ultra-blue. As the sodium-vapour becomes less dense, more light passes through, and the same absorption-bands are seen as are observed in the other method. The vapour then has a slight bluish-green tint, but is nearly colourless.

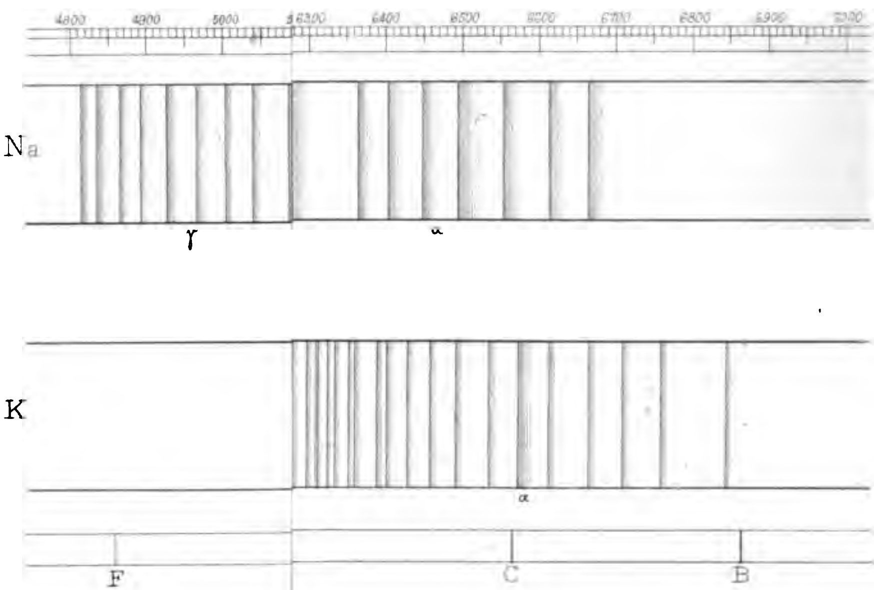
The following numbers give the wave-lengths of the more refrangible edge of the sodium absorption-bands in tenth-metres, obtained in the manner above described:—

$$\left.\begin{matrix}6668\\6616\\6552\\6499\\6450\\6405\end{matrix}\right\}\alpha \qquad \left.\begin{matrix}6361\\6272\\6235\\6192\\6162\\6149\end{matrix}\right\}\alpha \qquad \left.\begin{matrix}6105\\6092\\6071\\6051\\6035\\6016\end{matrix}\right\}\beta \qquad \begin{matrix}5999\ \ \beta\\ \left.\begin{matrix}5150\\5129\\5082\\5038\\5002\end{matrix}\right\}\gamma\end{matrix} \qquad \left.\begin{matrix}4964\\4927\\4889\\4863\\4832\\4810\end{matrix}\right\}\gamma$$

Plate IV. shows the general appearance of the two absorption-spectra.

II. "Note on the alleged Existence of Remains of a Lemming in Cave-deposits of England." By Professor Owen, C.B., F.R.S. Received April 25, 1874.

In the "Report on the Exploration of Brixham Cave" (Phil. Trans. 1873) it is stated (p. 560):—"With the appearance in the cave of the smaller common rodents now living in this country, we have to note a remarkable exception, that of the Lemming (*Lagomys spelæus*)." And again, in the list of animal remains as determined by Dr. Falconer and by



W. H. Wesley lith.

W. West & Co imp.

Mr. Busk, there occurs (p. 556):—"16. *Lagomys spelæus.* Lemming . . 1." This is throughout the "Report" treated as an original discovery, the importance of which is impressed upon the Royal Society by the remark:—"This circumstance tends to give a greater antiquity to a portion of the smaller remains than from their condition and position we might have been disposed to assign to them" (*ib.* p. 560, note). These remains are referred to "the smaller common rodents now living in this country," viz. "Hare, Rabbit, Water-rats," "at least two species of *Arvicola*" (*ib.* p. 548).

The supposed existence of remains of a Grisly Bear in the Brixham Cave (Mr. Busk having "reason to believe that bear-remains referred to *Ursus priscus* belong in fact to *Ursus ferox*"—an "important determination") leads to the remark:—"The presence of another small North-American animal has been ascertained, viz. the Lemming" (*ib.* p. 556).

At the date of publication of my 'British Fossil Mammals,' it is true that no fossil evidence of a Lemming (*Georychus*, Illiger; *Lemmus*, Link) had come to my knowledge; but I have since obtained such of species of both *Spermophilus* and *Georychus*, the latter nearly allied to, if not identical with, the Siberian Lemming (*Georychus aspalax*), from a deposit of lacustrine brick-earth near Salisbury, associated with *Elephas primigenius*. The Lemmings, I may remark, belong to the family of "Voles" (*Arvicolidæ*), not of "Hares" (*Leporidæ*); but the fossil from "the surface of the cave-earth far in the Reindeer gallery" of the Brixham Cave (Report, p. 558) appears from the figures (plate xlvi. figs. 12, 13) to be rightly referred to *Lagomys*, and to the same species determined and named (p. 213, figs. 82, 83, 84) in the 'British Fossil Mammals' (1846). The specimen submitted to me by Dr. Buckland was found by the Rev. Mr. M'Enery in Kent's Hole, Torquay, and includes a larger proportion of the skull than the specimen figured in the "Report" from the Brixham Cave. It is evidently a Pika, or tailless Hare, not a Lemming. And the determination of the original or first evidence of *Lagomys spelæus*, now in the British Museum, led me also to remark:—"None of the circumstances attending its discovery, nor any character deducible from its colour or chemical state, indicate it to be an older fossil than the jaws and teeth of the Hares, Rabbits, Field-voles, or Water-voles already described; yet it unquestionably attests the former existence in England of a species of rodent, whose genus not only is unrepresented at the present day in our British fauna, but has long ceased to exist in any part of the Continent of Europe" ('British Fossil Mammals,' p. 213). The Lemmings still disturb, by their multitudinous migratory swarms, the husbandmen of Scandinavia.

III. "On the alleged Expansion in Volume of various Substances in passing by Refrigeration from the state of Liquid Fusion to that of Solidification." By Robert Mallet, C.E., F.R.S. Received April 28, 1874.

(Abstract.)

Since the time of Réaumur it has been stated, with very various degrees of evidence, that certain metals expand in volume at or near their points of consolidation from fusion. Bismuth, cast iron, antimony, silver, copper, and gold are amongst the number, and to these have recently been added certain iron furnace-slags. Considerable physical interest attaches to this subject from the analogy of the alleged facts to the well-known one that water expands between 39° F. and 32°, at which it becomes ice; and a more extended interest has been given to it quite recently by Messrs. Nasmyth and Carpenter having made the supposed facts, more especially those relative to cast iron and to slags, the foundation of their peculiar theory of lunar volcanic action as developed in their work, 'The Moon as a Planet, as a World, and a Satellite' (4to, London, 1874). There is considerable ground for believing that bismuth does expand in volume at or near consolidation; but with respect to all the other substances supposed to do likewise, it is the object of this paper to show that the evidence is insufficient, and that with respect to cast iron and to the basic silicates constituting iron slags, the allegation of their expansion in volume, and therefore that their density when molten is greater than when solid, is wholly erroneous. The determination of the specific gravity, in the liquid state, of a body having so high a fusing temperature as cast iron is attended with many difficulties. By an indirect method, however, and operating upon a sufficiently large scale, the author has been enabled to make the determination with considerable accuracy. A conical vessel of wrought iron of about 2 feet in depth and 1·5 foot diameter of base, and with an open neck of 6 inches in diameter, being formed, was accurately weighed empty, and also when filled with water level to the brim; the weight of its contents in water, reduced to the specific gravity of distilled water at 60° F., was thus obtained. The vessel being dried was now filled to the brim with molten grey cast iron, additions of molten metal being made to maintain the vessel full until it had attained its maximum temperature (yellow heat in daylight) and maximum capacity. The vessel and its content of cast iron when cold were weighed again, and thus the weight of the cast iron obtained. The capacity of the vessel when at a maximum was calculated by applying to its dimensions at 60° the expansion calculated from the coefficient of linear dilatation, as given by Laplace, Riemann, and others, and from its range of increased temperature; and the weight of distilled water held by the vessel thus ex-

panded was calculated from the weight of its contents when the vessel and water were at 60° F.

We have now, after applying some small corrections, the elements necessary for determining the specific gravity of the cast iron which filled the vessel when in the molten state, having the absolute weights of equal volumes of distilled water at 60° and of molten iron. The mean specific gravity of the cast iron which filled the vessel was then determined by the usual methods. The final result is that, whereas the specific gravity of the cast iron at 60° F. was 7·170, it was only 6·650 when in the molten condition; cast iron, therefore, is less dense in the molten than in the solid state. Nor does it expand in volume at the instant of consolidation, as was conclusively proved by another experiment. Two similar 10-inch spherical shells, 1·5 inch in thickness, were heated to nearly the same high temperature in an oven, one being permitted to cool empty as a measure of any permanent dilatation which both might sustain by mere heating and cooling again, a fact well known to occur. The other shell, when at a bright red heat, was filled with molten cast iron and permitted to cool, its dimensions being taken by accurate instruments at intervals of 30 minutes, until it had returned to the temperature of the atmosphere (53° F.), when, after applying various corrections, rendered necessary by the somewhat complicated conditions of a spherical mass of cast iron losing heat from its exterior, it was found that the dimensions of the shell, whose interior surface was in perfect contact with that of the solid ball which filled it, were, within the limit of experimental error, those of the empty shell when that also was cold (53° F.), the proof being conclusive that no expansion in volume of the contents of the shell had taken place. The central portion was much less dense than the exterior, the opposite of what must have occurred had expansion in volume on cooling taken place.

It is a fact, notwithstanding what precedes, and is well known to iron-founders, that certain pieces of cold cast iron do float on molten cast iron of the same quality, though they cannot do so through their buoyancy. As various sorts of cast iron vary in specific gravity at 60° F., from nearly 7·700 down to 6·300, and vary also in dilatability, some cast irons may thus float or sink in molten cast iron of different qualities from themselves through buoyancy or negative buoyancy alone; but where the cold cast iron floats upon molten cast iron of less specific gravity than itself, the author shows that some other force, the nature of which yet remains to be investigated, keeps it floating; this the author has provisionally called the repellent force, and has shown that its amount is, *cæteris paribus*, dependent upon the relation that subsists between the volume and "effective" surface of the floating piece. By "effective" surface is meant all such part of the immersed solid as is in a horizontal plane or can be reduced to one. The repellent force has also relations to the difference in temperature between the solid and the molten metal on which it floats.

The author then extends his experiments to lead, a metal known to contract greatly in solidifying, and, with respect to which, no one has suggested that it expands at the moment of consolidation. He finds that pieces of lead having a specific gravity of 11·361, and being at 70° F., float or sink upon molten lead of the same quality, whose calculated specific gravity was 11·07, according to the relation that subsists between the volume and the "effective" surface of the solid piece, thin pieces with large surface always floating, and *vice versâ*. An explanation is offered of the true cause of the ascending and descending currents observed in very large "ladles" of liquid cast iron, as stated by Messrs. Nasmyth and Carpenter. The facts are shown to be in accordance with those above mentioned, and when rightly interpreted to be at variance with the views of these authors.

Lastly, the author proceeds to examine the statements made by these writers, as to the floating of lumps of solidified iron furnace-slag upon the same when in a molten state; he examines the conditions of the alleged facts, and refers to his own experiments upon the total contraction of such slags, made at Barrow Iron-works (a full account of which he has given in his paper on "The true Nature and Origin of Volcanic Heat and Energy," printed in Phil. Trans. 1873), as conclusively proving that such slags are not denser in the molten than in the solid state, and that the floating referred to is due to other causes. The author returns thanks to several persons for facilities liberally afforded him in making these experiments.

IV. "Note on the Excitation of the Surface of the Cerebral Hemispheres by Induced Currents." By J. Burdon Sanderson, M.D., F.R.S., Professor of Practical Physiology in University College, London. Received April 30, 1874.

In a paper recently communicated to the Royal Society by Dr. Ferrier (Proceedings, No. 151) it is shown that when two ends of copper wire distant from each other not more than a couple of millimetres, and in metallic communication with the terminals of the secondary coil of a Du Bois's induction-apparatus in action, are applied to certain spots of the surface of either hemisphere, and great intensity is given to the induced currents thereby directed through the living tissue, by previously bringing the secondary coil into such a position that it is very close to the primary coil or even partially covers it, characteristic combined movements of the opposite side of the body are produced.

With reference to these effects, it was observed by Dr. Ferrier (1) that excitation of the same spot always produces the same movement in the same animal, (2) that the area of excitability for any given movement (or, as it may be called for shortness, *the active spot*) is extremely small and admits of very accurate definition, and (3) that in different animals

excitations of anatomically corresponding spots produce similar or corresponding results. From these remarkable facts and from others similar to them relating to other parts of the brain to which I do not now advert, it was inferred that, at the surface of the hemispheres, certain "centres" are to be found, of which it is the function to originate combined or even purposive movements.

To this inference objections have been recently raised by Dr. Dupuy, based on the results of experiments made by him, in which he found that, after the ablation of those parts of the hemispheres which contain the supposed centres, movements, similar to those described by Dr. Ferrier, can still be produced by electrical excitation of the cut surface. In commenting on these counter experiments, Dr. Ferrier has since pointed out that the effects described by Dr. Dupuy are entirely different from those observed by himself, and, particularly, that the movements produced in his experiments are of an uncertain character, affecting sometimes one, sometimes several groups of muscles.

As it appeared to me that, although Dr. Dupuy has failed to prove that the movements he described are of the same nature with those described by Dr. Ferrier, the latter has not proved that they are different, I thought it necessary to make a series of experiments for the purpose of clearing up this uncertainty. With this view I determined to investigate the most characteristic of the combined movements, so accurately described by Dr. Ferrier as produced by excitation of particular spots on the anterior part of either hemisphere, by comparing them with those produced by excitation of deeper parts. The results of my experiments, in which cats were employed, are as follows:—

1. By removing the integument, skull, and dura mater to an extent corresponding to the anterior half of the right parietal bone and the adjoining thin portion of the frontal bone, an area of the surface of the brain is brought into view which comprises several spots by the excitation of which the following characteristic movements can be produced:— (1) Retraction of the left fore paw, with flexion of the carpus, accompanied by similar movements of the left hind leg. (2) Closure of the left eye and elevation of the left upper lip. (3) Retraction of the left ear. (4) Rotation of the head to the left side.

The active spots for these several movements are as follows:—For (1), a point immediately behind the outer end of the crucial sulcus; for (2), the surface about the outer end of a sulcus which lies immediately behind (1); for (3), the surface behind the sulcus last mentioned; for (4), a spot about a centim. further back on the same convolution. Movements (1), (2), and (3) can be produced in the cat with very great certainty, and the active spots for them are well defined. Their limits and relations are in exact accordance with the statements of Dr. Ferrier.

2. If that part of the surface of the right hemisphere which comprises the active spots above mentioned is severed from the deeper parts by a

nearly horizontal incision made with a thin-bladed knife, and the instrument is at once withdrawn, without dislocation of the severed part, and the excitation of the active spots thereupon repeated, the result is the same as when the surface of the uninjured organ is acted upon.

If a similar incision is made in a parallel plane, but at a lower level, this is not the case; but on removing the flap and applying the electrodes to the cut surface, it is found that there are on it active spots, which, as regards the effect of excitation, have the same properties as the active spots previously observed on the natural surface, and that the latter have the same topographical relation to each other as the former.

3. In a brain hardened in alcohol a needle plunged vertically, *i. e.* at right angles to the surface, from the active spot for retraction of the opposite ear, reaches the posterior part of the *corpus striatum* at a depth of from 10 to 12 millims. If a horizontal incision is made in the living brain, at this depth, and is met by two others, of which one is directed antero-posteriorly and the other transversely, and the part comprised within the incisions removed, a surface of brain is exposed in the deepest part of the wound which corresponds to the outer and upper part of the *corpus striatum* *. If now the electrodes are applied to this surface, the movements (1), (2), (3) are produced in the same way as before, but more distinctly; the active spots are quite as strictly localized, and their relations to each other are the same as at the surface—the spot for the movement of the extremities being in front, that for the closure of the eye and retraction of the upper lip being to the outside, and that for the ear behind.

From these facts it appears that the superficial convolutions do not contain organs which are essential to the production of the combinations of muscular movements now in question. They further make it probable that the doctrine hitherto accepted by physiologists, that the centres for such movements are to be found in the masses of grey matter which lie in the floor and outer wall of each lateral ventricle, is true.

* In case it should be necessary to repeat this experiment, it will be found best (after having noted the effects of exciting the surface at the several active spots and ascertained the degree of excitation required for the production of the corresponding movements) to proceed to remove the part of the brain containing them, so as to expose the outer aspect of the anterior part of the *corpus striatum* at once; and then, as soon as hæmorrhage has ceased, to investigate the relative positions of the active spots on the surface so exposed. [Since the above paper was communicated, I have ascertained that at the lowest part of this surface there is a spot, of which excitation induces opening of the mouth and alternate protrusion and retraction of the tongue—a group of movements which Dr. Ferrier has localized on the under surface of the brain, in front of the Sylvian fissure.—J. B. S., June 3, 1874.]

V. "Spectroscopic Notes.—No. I. On the Absorption of great Thicknesses of Metallic and Metalloidal Vapours." By J. NORMAN LOCKYER, F.R.S. Received April 20, 1874.

It has been assumed hitherto that a great *thickness* of a gas or vapour causes its radiation, and therefore its absorption, to assume more and more the character of a continuous spectrum as the thickness is increased.

It has been shown by Dr. Frankland and myself that such a condition obtains when the *density* of a vapour is increased, and my later researches have shown that it is brought about in two ways. Generalizing the work I have already done, without intending thereby to imply necessarily that the rule will hold universally, or that it exhausts all the phenomena, it may be stated that metallic elements of low specific gravity approach the continuous spectrum by widening their lines, while metallic elements of high specific gravity approach the continuous state by increasing the number of their lines. Hence in the vapours of Na, Ca, Al, and Mg we have a small number of lines which broaden, few short lines being added by increase of density; in Fe, Co, Ni, &c. we have many lines which do not so greatly broaden, many short lines being added.

The observations I made in India during the total solar eclipse of 1871 were against the assumption referred to; and if we are to hold that the lines, both "fundamental" and "short," which we get in a spectrum, are due to atomic impact (defining by the word atom, provisionally, that mass of matter which gives us a line-spectrum), then, as neither the quantity of the impacts nor the quality is necessarily altered by increasing the thickness of the stratum, the assumption seems also devoid of true theoretical foundation.

One thing is clear, that if the assumed continuous spectrum is ever reached by increased thickness, as by increased density, it must be reached through the "short-line" stage.

To test this point I have made the following experiments :—

1. An iron tube about 5 feet long was filled with dry hydrogen; pieces of sodium were carefully placed at intervals along the whole length of the tube, except close to the ends. The ends were closed with glass plates. The tube was placed in two gas-furnaces in line and heated. An electric lamp was placed at one end of the tube and a spectroscope at the other.

When the tube was red-hot and filled with sodium-vapour throughout, as nearly as possible, its whole length, a stream of hydrogen slowly passing through the tube, the line D was seen to be absorbed; it was no thicker than when seen under similar conditions in a test-tube, and far thinner than the line absorbed by sodium-vapour in a test-tube, if the density be only slightly increased.

Only the longest "fundamental" line was absorbed.

The line was thicker than the D line in the solar spectrum, in which spectrum all the short lines are reversed.

2. As it was difficult largely to increase either the temperature or the density of the sodium-vapour, I have made another series of experiments with iodine-vapour.

I have already pointed out the differences indicated by the spectroscope between the quality of the vibrations of the "atom" of a metal and of the "subatom" of a metalloid (by which term I define that mass of matter which gives us a spectrum of channelled spaces, and builds up the continuous spectrum in its own way). Thus, in iodine, the short lines, brought about by increase of density in an atomic spectrum, are represented by the addition of a system of well-defined "beats" and broad bands of continuous absorption to the simplest spectrum, which is one exquisitely rhythmical, the intervals increasing from the blue to the red, and in which the beats are scarcely noticeable.

On increasing the density of a very small thickness by a gentle heating, the beats and bands are introduced, and, as the density is still further increased, the absorption becomes continuous throughout the whole of the visible spectrum.

The absorption of a thickness of 5 feet 6 inches of iodine-vapour at a temperature of 59° F. has given me no indication of bands, while the beats were so faint that they were scarcely visible.

VI. "Spectroscopic Notes.—No. II. On the Evidence of Variation in Molecular Structure." By J. Norman Lockyer, F.R.S. Received May 26, 1874.

1. In an accompanying note I have shown that when different degrees of dissociating power are employed the spectral effects are different.

2. In the present note I propose to give a preliminary account of some researches which have led me to the conclusion that, starting with a mass of elemental matter, such mass of matter is continually broken up as the temperature (including in this term the action of electricity) is raised.

3. The evidence upon which I rely is furnished by the spectroscope in the region of the visible spectrum.

4. To begin by the extreme cases, all solids give us continuous spectra; all vapours produced by the high-tension spark give us line-spectra.

5. Now the continuous spectrum may be, and as a matter of fact is, observed in the case of chemical compounds, whereas all compounds known as such are resolved by the high-tension spark into their constituent elements. We have a right, therefore, to assume that an element in the solid state is a more complex mass than the element in a state of vapour, as its spectrum is the same as that of a mass which is known to be more complex.

6. The spectroscope supplies us with intermediate stages between these extremes.

(α) The spectra vary as we pass from the induced current with the jar to the spark without the jar, to the voltaic arc, or to the highest temperature produced by combustion. The change is always in the same direction; and here, again, the spectrum we obtain from elements in a state of vapour (a spectrum characterized by spaces and bands) is similar to that we obtain from vapours of which the compound nature is unquestioned.

(β) At high temperatures, produced by combustion, the vapours of some elements (which give us neither line- nor channelled space-spectra at those temperatures, although we undoubtedly get line-spectra when electricity is employed, as stated in 4) give us a continuous spectrum at the more refrangible end, the less refrangible end being unaffected.

(γ) At ordinary temperatures, in some cases, as in selenium, the more refrangible end is absorbed; in others the continuous spectrum in the blue is accompanied by a continuous spectrum in the red. On the application of heat, the spectrum in the red disappears, that in the blue remains; and further, as Faraday has shown in his researches on gold-leaf, the masses which absorb in the blue may be isolated from those which absorb in the red. It is well known that many substances known to be compounds in solution give us absorption in the blue or blue and red; and, also, that the addition of a substance known to be compound (such as water) to substances known to be compound which absorb the blue, superadds an absorption in the red.

7. In those cases which do not conform to what has been stated the limited range of the visible spectrum must be borne in mind. Thus I have little doubt that the simple gases, at the ordinary conditions of temperature and pressure, have an absorption in the ultra-violet, and that highly compound vapours are often colourless because their absorption is beyond the red, with or without an absorption in the ultra-violet. Glass is a good case in point; others will certainly suggest themselves as opposed to the opacity of the metals.

8. If we assume, in accordance with what has been stated, that the various spectra to which I have referred are really due to different molecular aggregations, we shall have the following series, going from the more simple to the more complex:—

First stage of complexity of molecule	Line-spectrum.
Second stage..........	Channelled space-spectrum.
Third stage	Continuous absorption at the blue end not reaching to the less refrangible end. (This absorption may break up into channelled spaces.)

Fourth stage..........	Continuous absorption at the red end not reaching to the more refrangible end. (This absorption may break up into channelled spaces.)
Fifth stage	Unique continuous absorption.

9. I shall content myself in the present note by giving one or two instances of the passage of spectra from one stage to another, beginning at the fifth stage.

From 5 to 4.

1. The absorption of the vapours of K in the red-hot tube, described in another note, is at first continuous. As the action of the heat is continued, this continuous spectrum breaks in the middle; one part of it retreats to the blue, the other to the red.

From 4 to 3.

1. Faraday's researches on gold-leaf best illustrate this; but I hold that my explanation of them by masses of two degrees of complexity only is sufficient without his conclusion ('Researches in Chemistry,' p. 417), that they exist "of intermediate sizes or proportions."

From 3 to 2.

1. Sulphur-vapour first gives a continuous spectrum at the blue end; on heating, this breaks up into a channelled space-spectrum.

2. The new spectra of K and Na (more particularly referred to in the third note) make their appearance after the continuous absorption in the blue and red vanishes.

From 2 to 1.

1. In many metalloids the spectra, without the jar, are channelled; on throwing the jar into the circuit the line-spectrum is produced, while the cooler exterior vapour gives a channelled absorption-spectrum.

2. The new spectra of K and Na change into the line-spectrum (with thick lines which thin subsequently) as the heat is continued.

VII. "Spectroscopic Notes.—No. III. On the Molecular Structure of Vapours in connexion with their Densities." By J. Norman Lockyer, F.R.S. Received May 26, 1874.

1. I have recently attempted to bring the spectroscope to bear upon the question whether vapours of elements below the highest temperatures are truly homogeneous, and whether the vapours of different chemical elements, at any one temperature, are all in a similar molecular condition. In the present note, I beg to lay before the Royal Society the preliminary results of my researches.

2. We start with the following facts :—

I. All elements driven into vapour by the induced current give line-spectra.

II. Most elements driven into vapour by the voltaic arc give us the same.

III. Many metalloids when greatly heated, some at ordinary temperatures, give us channelled-space spectra.

IV. Elements in the solid state give us continuous spectra.

3. If we grant that the spectra represent to us the vibrations of different molecular aggregations (this question is discussed in Note II.), spectroscopic observations should furnish us with facts of some importance to the inquiry.

4. To take the lowest ground. If, in the absence of all knowledge on the subject, it could be shown that all vapours at all stages of temperature had spectra absolutely similar in character, then it would be more likely that all vapours were truly homogeneous and similar among themselves, as regards molecular condition, than if the spectra varied in character, not only from element to element, but from one temperature to another in the vapour of the same element.

5. At the temperature of the sun's reversing layer, the spectra of all the elements known to exist in that layer are apparently similar in character—that is, they are all line-spectra; hence it is more probable that the vapours there are truly homogeneous, and that they all exist in the same molecular condition, than if the spectrum were a mixed one.

6. The fact that the order of vapour-densities in the sun's atmosphere, which we can in a measure determine by spectroscopic observations, does not agree with the order of the modern atomic weights of the elements, but more closely agrees with the older atomic weights, led me to take up the present research. Thus I may mention that my early observations of the welling-up of Mg vapour all round the sun *above the* Na *vapour* have lately been frequently substantiated by the Italian observers; so that it is beyond all question, I think, that, *at the sun*, the vapour-density of Mg is less than that of Na.

7. The vapour-densities of the following elements have been experimentally determined :—

H	1	S	32 (at 1000°)
K	39	I	127
As	150	Hg	100
Br	80	N	14
Cd	56	O	16
Cl	35·5	P	62

8. To pursue this inquiry the following arrangements have been adopted :—

The first experiments were made last December upon Zn in a glass

tube closed at each end with glass plates; and I have to express my obligations to Dr. Russell for allowing them to be conducted in his laboratory, and for much assistance and counsel concerning them.

A stream of dry H was allowed to pass. The tube was heated in a Hofmann's gas-furnace, pieces of the metal to be studied having previously been introduced. It was found that the glass tube melted; it was therefore replaced by an iron one. The inconvenience of this plan, however (owing to the necessity for introducing the metal into the end of the hot tube when the first charge had volatilized), and, moreover, the insufficiency of the heat obtainable from the gas-furnace, soon obliged me to replace both tube and furnace by others, which have now been in use for many weeks, and which still continue to work most satisfactorily.

The iron tube is 4 feet in length, and is provided with a central enlargement, suggested to me by Mr. Dewar, forming a T-piece by the screwing in of a side tube, the end of which is left projecting from the door in the roof of the furnace. Caps are screwed on at each end of the main tube; these caps are closed by a glass plate at one end, and have each a small side tube for the purpose of passing hydrogen or other gases through the hot tube. The furnace is supplied with coke or charcoal; an electric lamp, connected with thirty Grove's cells, is placed at one end of the tube and a one-prism spectroscope at the other. The temperatures reached by this furnace may be conveniently divided into four stages:—

I. When the continuous spectrum of the tube extends to the sodium-line D, this line not being visible.

II. When the continuous spectrum extends a little beyond D, this line being visible as a bright line.

III. When the spectrum extends into the green, D being very bright.

IV. When the spectrum extends beyond the green and D becomes invisible as a line, and the sides of the furnace are at a red heat.

I may add (1) that I have only within the last few days been able to employ the third and fourth stages of heat, as the furnace was previously without a chimney, and the necessary draught could not be obtained; and (2) that I was informed, a little time ago, by Prof. Roscoe that, with a white-hot tube, he had observed new spectra in the case of Na and K. These spectra, which I now constantly see when these temperatures are reached, I shall call the "new spectra."

9. The results of the experiments, so far as the visible spectrum is concerned, between the stages indicated may be stated as follows:—

H. No absorption.

N. No absorption.

K. I have observed, either separately or together:—

(α) The line absorption-line near D.

(β) Continuous absorption throughout the whole spectrum.

(γ) Continuous absorption in red and blue at the same time,

the light being transmitted in the centre of the spectrum (as by gold-leaf).

(δ) Continuous absorption clinging on one side or other of the line. (This phenomenon, which, so far as I know, is quite new, will be described in another note.)

(ϵ) The new spectrum.

Na. I have observed, either separately or together:—

(α) D absorbed.

(β) Continuous absorption throughout the whole spectrum.

(γ) Continuous absorption clinging on one side or the other of D.

(δ) The new spectrum.

Zn. Continuous absorption in the blue. (An unknown line sometimes appears in the green, but certainly no line of Zn.)

Cd. Continuous absorption in the blue.

Sb. New spectrum, with channelled spaces and absorption in the blue.

P. The same. (This, however, in consequence of the extreme delicacy of the spectrum, requires confirmation.)

S. Channelled-space spectrum (previously observed by Salet).

As. Probable channelled-space spectrum. (Observations to be repeated.)

Bi. No absorption.

I. Channelled spectrum in the green and intense bank of general absorption in the violet, where at the ordinary temperature the vapour transmits light.

Hg. No absorption.

10. These results may be tabulated as follows:—

	V. d.	Modern atomic weight.	
H	1	1	No visible absorption.
K	39	39	Line absorption.
As	150	75	Probable channelled-space absorption.
Cd	56	112	Continuous absorption in the blue.
I	127	127	Channelled-space absorption + band of absorption in violet.
Hg	100	200	No absorption.
N	14	14	,, ,,
P	62	31	Channelled-space spectrum probable.
Na	(?)	23	Line absorption.
Zn	(?)	65	Continuous absorption in the violet.
Sb	(?)	122	Channelled-space spectrum and absorption in the blue.
S	32	32	Channelled-space spectrum.
Bi	(?)	208	No absorption.

11. It will be seen from the foregoing statement that if similar spectra be taken as indicating similar molecular conditions, then the vapours, the densities of which have been determined, have not been in the same molecular condition among themselves. Thus the vapours of K, S, and Cd, at the fourth stage of heat, gave us line, channelled-space, and continuous absorption in the blue respectively. This is also evidence that each vapour is non-homogeneous for a considerable interval of time, the interval being increased as the temperature is reduced.

VIII. "Spectroscopic Notes.—No. IV. On a new Class of Absorption Phenomena." By J. Norman Lockyer, F.R.S. Received May 26, 1874.

1. In the experiments on the absorption-spectrum of Na and K vapour heated in a red-hot tube, to which further reference is made in separate notes, I have observed phenomena quite new to me, some rough drawings of which I lay herewith before the Royal Society. As the phenomena are only momentary, I cannot answer for the final accuracy of the drawings, nor have I been able to represent the softness of the gradations of shade.

2. In the drawings, the red end of the spectrum is to the left; the D line common to them all is the image of a slit about half an inch long, on which slit the light falls from an electric lamp, through the tube and chamber in which the vapours are produced. The lower part of the drawings would generally represent, therefore, the spectrum of the *less dense vapours* were the vapours at rest.

3. One of the phenomena referred to consists of what may be described as a unilateral widening of the line D: the side absorption, however, is much less dense than that of the line; it is bounded by D on one side and by a curved line on the other. Figs. 1, 2, and 3 will give an idea of this appearance in three stages as it is frequently actually seen, *i. e.* as the absorption travels up or down the line it widens as shown.

Fig. 1.

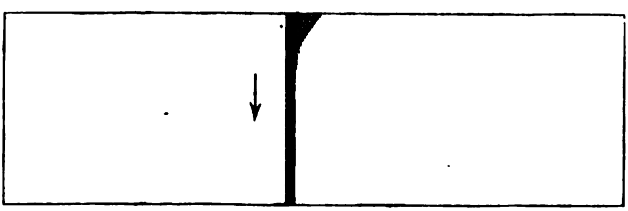


Fig. 2.

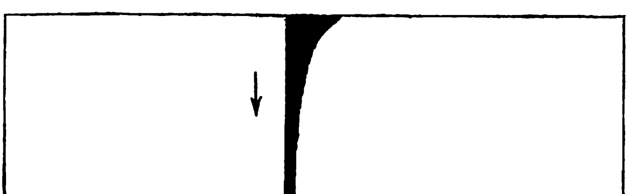
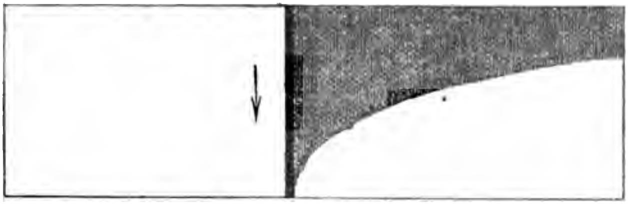


Fig. 3.


4. Figs. 4 and 5 give two variations sometimes observed—fig. 4 showing the darkening in the absorption and an increased steepness in the curve; fig. 5 the simultaneous existence of apparently different absorptions, all bounded by D on one side, but by different curves on the other, and being of different intensities.

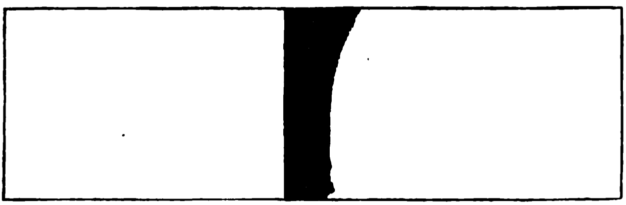
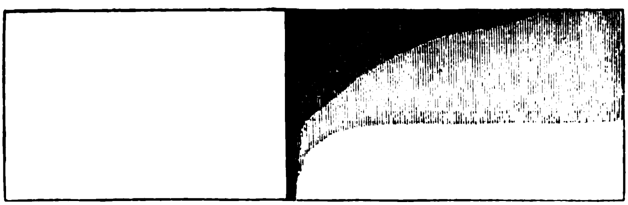
Fig. 4.


Fig. 5.


5. Although, in the preceding drawings, I have represented this unilateral widening exclusively on the more refrangible side of D, I have observed it on the other, though scarcely so frequently.

6. Accompanying these appearances, but generally best visible when the absorption with curved boundary is visible on both sides of D, is a brilliant boundary replacing the mere change of shade.

7. At times the brilliant boundary is continuous across D, as shown in fig. 6; but I append figs. 7 and 8 to show that the phenomena on either side of D are independent of each other.

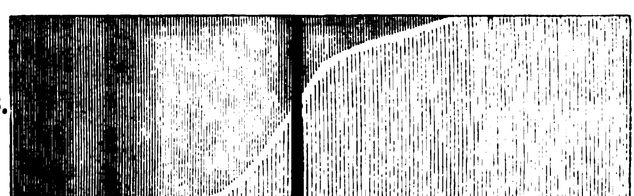
Fig. 6.


Fig. 7.

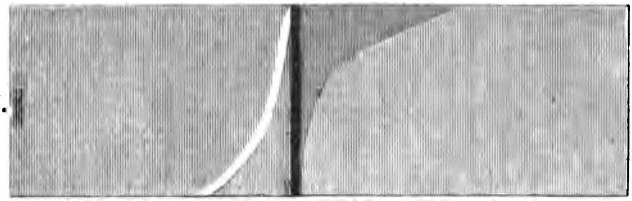
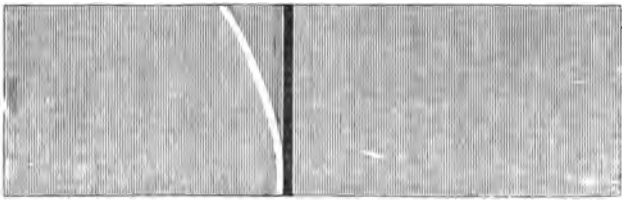


Fig. 8.



8. At times, D puts on the appearance of the limiting line of a channelled-space spectrum, the "easing off" of the absorption being now on one side and now on the other.

9. Should all these phenomena be ultimately referred to the causes which produce a channelled-space spectrum (one of which undoubtedly is the tendency to a unilateral instead of a bilateral widening), a line-spectrum will be regarded as a special case merely, and not as an entirely different spectrum, as it has been hitherto; and the range of molecular combinations in any one element from which line-spectra may be produced is extended.

10. The question further arises, whether many of the short lines in spectra are not remnants of channelled-space spectra.

June 18, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

Mr. Henry Bowman Brady, Mr. Augustus Wollaston Franks, Prof. Olaus Henrici, Sir Henry Sumner Maine, and Mr. Osbert Salvin were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A Contribution to the Anatomy of Connective Tissue, Nerve, and Muscle, with special reference to their connexion with the Lymphatic System." By G. Thin, M.D. Communicated by Prof. Huxley, Sec. R.S. Received April 22, 1874*.

* This Paper will appear in No. 155.

II. "Given the Number of Figures (not exceeding 100) in the Reciprocal of a Prime Number, to determine the Prime itself." By WILLIAM SHANKS. Communicated by the Rev. G. SALMON, F.R.S. Received May 19, 1874.

In a former communication (*suprà*, p. 200) I gave a Table showing the number of figures in the period of the reciprocal of every given prime up to 20,000. The Table here introduced is intended to solve the converse problem, and to show what primes have a given number of figures in their period. It appears at once, from the ordinary rule for converting a pure circulating decimal into a proper fraction, that if the reciprocal of a prime have n figures in its period, that prime must be a factor in the number formed by writing down n nines, and therefore also, generally, in the number formed by writing down n ones. We denote that number by n; that is to say, 5 (in the left column), for example, $=11111$, except where $3, 3^2, 3^3 \ldots . 3^6$ are concerned, when we have 3, for example, $=999$. The problem now before us is equivalent to that of breaking up n into its prime factors; and the previous Table gives us great facility in doing this, for it exhibits every factor of n which is less than 20,000*; and if, after accounting for all these, the remaining factor of n is less than $30{,}000^2$, we may be sure that it is a prime number, and that the resolution is complete.

If we have to deal with a composite number mn, this may obviously be written down either as m groups of n ones or as n groups of m ones. It follows that mn contains m and n as factors. We may also state here that 12, besides the factor 9901, obviously has all the factors belonging to any submultiple of 12, *e.g.* 2, 3, 4, 6; and that this holds in all other similar cases, and need not be stated again. When we affirm that the resolution in any case is complete (and, indeed, throughout the Table), it is to be clearly understood that the submultiples have all been carefully attended to, and thus any result may easily be verified. The high factors found (those, we mean, above $30{,}000^2$) have involved considerable labour; and though we may not say absolutely that they are primes, yet we are certain that, if composite, their component factors are primes each greater than 30,000, and that the periods of their reciprocals have readily been found. It only remains to add here that the left column contains the given number of figures in the reciprocal of the prime or primes found and placed opposite in the right column, or, in a few cases, of the second powers of primes, and as far as the sixth power of the prime 3.

If the number of figures in the reciprocal of P be n, then the general rule†, which may be drawn from particular cases such as the following two, is that the number of figures in the reciprocal of P^2 is nP, of P^3 is

* In point of fact I have carried on the calculation up to 30,000.

† See 'Messenger of Mathematics,' vol. ii. pp. 41–43 (1872), and vol. iii. pp. 52–55 (1873).

nP^2, and so on. Since the period of $\frac{1}{19} \equiv 18$, and since the remainders resulting from dividing 18 such periods successively by 19 are, in order, 15, 11, 7, 3, 18, 14, 10, 6, 2, 17, 13, 9, 5, 1, 16, 12, 8, 4, 0, it follows that $\frac{1}{19^2} \equiv 18 \times 19 \equiv 342$. The law of such remainders, after the first has been obtained, is simple enough, and may be written down at once. Again, since the period of $\frac{1}{163} \equiv 81$, also since the remainders resulting from dividing 163 such periods, each of 81 figures, successively by 163 are, in order, 149, 135, 121, 107, 93, 79, 65, 51, 37, 23, 9, 158, 144....0 (the series consisting of 163 terms, of which the last is 0), it follows that $\frac{1}{163^2} \equiv 81 \times 163 \equiv 13203$. The law of the above series is evident, and the number of terms is easily found to be 163. There is an obvious exception when $P=3$; then the period is divisible by P, and the number of figures in the reciprocal of 3^2 is 1, of 3^3 is 3, and of 3^n is 3^{n-2}. There are other exceptions also, or at all events one. Desmarest, for instance, has remarked that in the case of $P=487$, the period is divisible by 487; and therefore the number of figures in the reciprocal of 487^2 is the same as that in the reciprocal of 487, viz. 486. I am not acquainted with the general theory of such exceptions; nor do I know what other primes (if any) besides 3 and 487 have the same peculiarity.

With these explanations the following Table can readily be understood. We mark with an asterisk those cases in which the resolution is complete, thus 28 | 29 . 281 . 12149 9449. We are to be understood as affirming that 12149 9449 is a prime number.

Given number of figures in Period of Primes.	Primes, Prime Factors, &c.
1*	3 and 3^2.
2*	11.
3*	3^3 . 37.
4*	101.
5*	41 . 271.
6*	7 . 13.
7*	239 . 4649.
8*	73 . 137.
9*	3^4 . 333667.
10*	9091.
11*	21649 . 513239.
12*	9901.
13*	53 . 79 . 26537 1653.
14*	90909 1.
15*	31 . 29061 61.
16*	17 . 58823 53.
17	~~Seems prime.~~ = 2071723 · 5363222357
18*	19 . 52579.
19*	~~Seems~~ prime.
20*	3541 . 27961.

Given number of figures in Period of Primes.	Primes, Prime Factors, &c.
21*	43 . 1933 . 10838 689.
22*	11^2 . 23 . 4093 . 8779.
23*	Seems prime.
24*	99990 001.
25	21401 . 25601 . 18252 12130 01.
26*	859 . 10583 13049.
27	3^5 . 757 . 44033 46547 77631.
28*	29 . 281 . 12149 9449.
29	3191 . 16763 . 20772 03000 95927 10406 7. = 43037 × 62003 × 77643639397
30*	211 . 241 . 2161.
31*	2791 . 39810 50201 04303 51526 73275 21.
32*	353 . 449 . 641 . 1409 . 69857.
33*	67 . 13446 28210 31329 8373.
34*	103 . 4013 . 21993 . 83336 9.
35*	71 . 12676 18436 74776 04353 521.
36*	99999 90000 01.
37*	Seems prime.
38*	90909 09090 90909 091.
39*	90090 09009 00990 99099 0991.
40*	99990 00099 99000 1.
41*	83 . 1231 . 10874 80167 08045 28702 40778 98379 32830 7.
42*	7^2 . 127 . 2689 . 45969 1.
43	173 . 64226 07578 67694 28387 92549 77520 87347 46307.
44	89 . 11124 70797 64156 1909.
45	99900 00009 99000 99999 9001.
46	47 . 139 . 2531 . 54979 71844 91917.
47	Seems prime.
48	99999 99900 00000 1.
49	10000 00100 00001 00000 01000 00010 00000 10000 001.
50	251 . 5051 . 78875 94347 2201.
51	613 . 14696 58892 17112 70961 00994 95907.
52	521 . 19003 81976 77733 22437 81.
53	107 . 10384 21599 16926 27206 64589 82346 83281 41225 33748 70197 3.
54	99999 99990 00000 001.
55	1321 . 68130 88570 01514 75398 18244 51098 41022 71. = 62921 ×
56	7841 . 12752 20010 20150 50376 1.
57	21319 . 42258 12190 53849 10220 50710 59144 89.
58	59 . 15408 32049 30662 55778 12018 49.
59	Seems prime.
60*	61 . 16557 36049 01641. = 4[illegible] × 39526741
61	733 . 4637 . 32690 11286 55567 78492 67785 60346 38966 63414 98113 99297 3391.
62	90909 09090 90909 09090 90909 09091.
63	10837 . 23311 . 39545 35794 55592 00238 00680 443. = 45613 ×
64	19841 . 50400 68544 93221 10780 70661 761.
65	90000 90000 90090 90090 90090 99090 99099 99099 991.
66	10989 01098 89010 98901 1.
67	Seems prime.
68	99009 90099 00990 09900 99009 90099 01.
69	277 . 32523 49822 74693 46602 92093 53758 09022 01840 83.
70	10999 88890 11110 98889 00011.
71	Seems prime.
72	3169 . 31555 69580 30609 02492 9. = 98641 ×
73	Seems prime.
74	7253 . 12533 99847 08521 86556 03324 01639 447.
75	151 . 4201 . 15763 98555 37391 91709 16417 09400 63151.
76	99009 90099 00990 09900 99009 90099 00990 1.
77	5237 . 17185 41321 38439 75575 73019 07599 58180 44493 01317 37618 86404 43. = 42043 ×
78	13^2 . 157 . 6397 . 84166 49699 61183 43.
79	317 . 6163 . 10271 . 55372 39794 64587 20397 50752 71926 68846 36072 32019 52048 12389 25326 15741 471.

Given number of figures in Period of Primes.	Primes, Prime Factors, &c.
80	99999 91000 00000 99999 99100 00000 1.
81	3^6 . 163 . 9397 . 21762 15574 17380 51978 03850 29334 29783 20758 07163 797.
82	90909 09090 90909 09090 90909 09090 90909 09091.
83	Seems prime.
84	10099 98990 00099 98990 00101.
85	90000 90000 90000 90900 90900 90900 90909 90909 90909 90909 99909 99909 9991.
86	90909 09090 90909 09090 90909 09090 90909 09090 91.
87	4003 . 22505 64329 00549 81286 55760 43195 08116 66025 25583 29000 997. =72559×
88	617 . 16205 83484 60129 67584 92708 26564 02106 953.
89	Seems prime.
90	29611 . ~~64209 50923 97453 00732~~ 9.
91	547 . 14197 . 17837 . 64973 58525 58248 78623 76372 29838 67691 22282 27693 73769 82738 03847 7.
92	1289 . 76811 40495 74080 75951 11722 18851 05500 46470 9.
93	90090 09009 00900 90090 09009 00900 99099 09909 90990 99099 09909 90991.
94	6299 . 14432 30527 21211 15905 84364 04046 81839 83027 609.
95	191 . 47120 89005 70681 09952 82727 69638 69115 13141 30942 35654 39842 88481 62827 17801. = 59281 × 63841 ×
96	97 . 10309 27835 05154 62886 59793 81443 3.
97	Seems prime.
98	197 . 50761 41624 36553 29949 18781 72639 59390 35533.
99	199 . 397 . 12657 74717 41579 43369 23914 28173 61378 68182 22092 83191 77752 74356 67. = 34849 ×
100	99999 99999 00000 00000 99999 99999 00000 00001. = 60101 ×

Note.—In the preparation of this paper valuable assistance was received from the Rev. Prof. Salmon, F.R.S., both in the way of suggestions and otherwise.—W. S.

Houghton-le-Spring,
April 18, 1874.

III. "On the Number of Figures in the Reciprocal of every Prime between 20,000 and 30,000." By William Shanks. Communicated by the Rev. George Salmon, F.R.S. Received June 6, 1874.

In a former communication* I gave the number of figures in the reciprocal of every prime below 20,000; the present Table is simply an extension of the former, and has been calculated by the same method. Towards the close of the former Table, viz. opposite the prime 19841, *instead* of 1984 *read* 64. The *whole* of the former Table has kindly been verified by the Rev. Dr. Salmon. For the accuracy of the following Table I am entirely responsible, and believe it is free from error.

* *Suprà*, p. 200.

In the left-hand columns of Table III. are primes; in the right-hand columns, immediately opposite, is the number of figures in the period of the reciprocal of each prime.

TABLE III. (*continued*).

20011	6670	20611	20610	21211	21210	21803	10901	22409	11204
20021	1540	20627	10313	21221	4244	21817	21816	22433	22432
20023	6674	20639	10319	21227	10613	21821	10910	22441	11220
20029	6676	20641	2580	21247	21246	21839	10919	22447	7482
20047	20046	20663	20662	21269	21268	21841	10920	22453	5613
20051	20050	20681	470	21277	1181	21851	21850	22469	22468
20063	20062	20693	739	21283	3547	21859	21858	22481	2810
20071	6690	20707	1479	21313	2368	21863	21862	22483	3747
20089	5022	20717	5179	21317	10658	21871	405	22501	7500
20101	20100	20719	3453	21319	57	21881	2188	22511	11255
20107	3351	20731	4146	21323	10661	21893	10946	22531	7510
20113	20112	20743	20742	21341	4268	21911	10955	22541	22540
20117	5029	20747	10373	21347	10673	21929	5482	22543	7514
20123	10061	20749	6916	21377	21376	21937	21936	22549	22548
20129	10064	20753	20752	21379	3054	21943	7314	22567	22566
20143	10071	20759	10379	21383	21382	21961	1220	22571	4514
20147	1439	20771	4154	21391	3565	21977	21976	22573	5643
20149	20148	20773	5193	21397	1783	21991	733	22613	5653
20161	1680	20789	20788	21401	25	21997	1833	22619	22618
20173	5043	20807	20806	21407	21406	22003	11001	22621	7540
20177	20176	20809	10404	21419	21418	22027	11013	22637	11318
20183	20182	20849	10424	21433	7144	22031	11015	22639	11319
20201	10100	20857	20856	21467	10733	22037	5509	22643	11321
20219	1838	20873	20872	21481	5370	22039	3673	22651	1510
20231	10115	20879	10439	21487	21486	22051	22050	22669	22668
20233	20232	20887	6962	21491	4298	22063	22062	22679	11339
20249	2531	20897	20896	21493	5373	22067	11033	22691	22690
20261	20260	20899	20898	21499	7166	22073	22072	22697	22696
20269	2252	20903	2986	21503	21502	22079	11039	22699	2522
20287	966	20921	2615	21517	5379	22091	4418	22709	22708
20297	20296	20929	436	21521	2152	22093	3682	22717	5679
20323	3387	20939	20938	21523	3587	22109	22108	22721	11360
20327	20326	20947	10473	21529	2691	22111	11055	22727	22726
20333	10166	20959	499	21557	5389	22123	3687	22739	22738
20341	4068	20963	10481	21559	10779	22129	5532	22741	22740
20347	10173	20981	4196	21563	10781	22133	5533	22751	11375
20353	20352	20983	20982	21569	10784	22147	11073	22769	11384
20357	5089	21001	250	21577	21576	22153	22152	22777	7592
20359	20358	21011	21010	21587	10793	22157	5539	22783	22782
20369	1273	21013	5253	21589	7196	22159	1231	22787	11393
20389	6796	21017	21016	21599	10799	22171	7390	22807	7602
20393	20392	21019	21018	21601	3600	22189	22188	22811	22810
20399	10199	21023	21022	21611	21610	22193	22192	22817	22816
20407	20406	21031	10515	21613	1801	22229	22228	22853	5713
20411	20410	21059	21058	21617	21616	22247	22246	22859	22858
20431	10215	21061	4212	21647	21646	22259	22258	22861	22860
20441	10220	21067	3511	21649	11	22271	11135	22871	11435
20443	3407	21089	10544	21661	7220	22273	22272	22877	5719
20477	5119	21101	21100	21673	21672	22277	5569	22901	22900
20479	10239	21107	10553	21683	10841	22279	3713	22907	11453
20483	10241	21121	10560	21701	21700	22283	11141	22921	3820
20507	10253	21139	7046	21713	21712	22291	22290	22937	22936
20509	20508	21143	21142	21727	21726	22303	22302	22943	22942
20521	1140	21149	21148	21737	21736	22307	11153	22961	5740
20533	10266	21157	5289	21739	7246	22343	11171	22973	11486
20543	20542	21163	3527	21751	375	22349	22348	22993	22992
20549	20548	21169	1323	21757	10878	22367	22366	23003	11501
20551	10275	21179	21178	21767	21766	22369	11184	23011	23010
20563	10281	21187	1177	21773	10886	22381	22380	23017	23016
20593	1872	21191	2119	21787	3631	22391	11195	23021	23020
20599	10299	21193	7064	21799	10899	22397	5599	23027	11513

For corrections [illegible] p. 261

TABLE III. (*continued*).

23029	23028	23669	23668	24229	24228	24971	24970	25621	25620
23039	11519	23671	11835	24239	12119	24977	24976	25633	25632
23041	5760	23677	11838	24247	24246	24979	8326	25639	4273
23053	1921	23687	23686	24251	24250	24989	24988	25643	12821
23057	2096	23689	3948	24281	2428	25013	6253	25657	25656
23059	23058	23719	11859	24317	12158	25031	12515	25667	25666
23063	23062	23741	23740	24329	12164	25033	1192	25673	25672
23071	11535	23743	23742	24337	24336	25037	12518	25679	12839
23081	11540	23747	11873	24359	12179	25057	25056	25693	6423
23087	23086	23753	23752	24371	24370	25073	25072	25703	25702
23099	23098	23761	11880	24373	4062	25087	25086	25717	2143
23117	5779	23767	23766	24379	8126	25097	25096	25733	12866
23131	23130	23773	5943	24391	1355	25111	12555	25741	2340
23143	23142	23789	23788	24407	24406	25117	6279	25747	4291
23159	11579	23801	2975	24413	6103	25121	12560	25759	25758
23167	7722	23813	5953	24419	24418	25127	1478	25763	12881
23173	11586	23819	23818	24421	1628	25147	4191	25771	1718
23189	2108	23827	11913	24439	12219	25153	25152	25793	25792
23197	1933	23831	11915	24443	24442	25163	12581	25799	12899
23201	464	23833	23832	24469	8156	25169	242	25801	2580
23203	11601	23857	7952	24473	24472	25171	5034	25819	1986
23209	5802	23869	23868	24481	6120	25183	25182	25841	2584
23227	11613	23873	23872	24499	24498	25189	8396	25847	25846
23251	4650	23879	11939	24509	24508	25219	8406	25849	718
23269	23268	23887	23886	24517	12258	25229	25228	25867	12933
23279	11639	23893	11946	24527	24526	25237	6309	25873	25872
23291	23290	23899	23898	24533	12266	25243	4207	25889	1618
23293	5823	23909	23908	24547	12273	25247	25246	25903	25902
23297	23296	23911	797	24551	2455	25253	12626	25913	25912
23311	63	23917	5979	24571	1638	25261	8420	25919	12959
23321	11660	23929	5982	24593	24592	25301	25300	25931	5186
23327	23326	23957	11978	24611	4922	25303	25302	25933	6483
23333	11666	23971	23970	24623	24622	25307	12653	25939	25938
23339	23338	23977	23976	24631	12315	25309	8436	25943	25942
23357	11678	23981	23980	24659	24658	25321	6330	25951	12975
23369	11684	23993	23992	24671	12335	25339	8446	25969	12984
23371	23370	24001	1000	24677	12338	25343	25342	25981	8660
23399	11699	24007	8002	24683	12341	25349	25348	25997	6499
23417	23416	24019	24018	24691	24690	25357	2113	26003	13001
23431	11715	24023	24022	24697	24696	25367	25366	26017	8672
23447	1234	24029	24028	24709	24708	25373	6343	26021	5204
23459	23458	24043	12021	24733	6183	25391	2539	26029	26028
23473	23472	24049	12024	24749	24748	25409	12704	26041	3255
23497	23496	24061	24060	24763	12381	25411	25410	26053	2171
23509	23508	24071	12035	24767	24766	25423	25422	26083	1449
23531	23530	24077	6019	24781	24780	25439	12719	26099	26098
23537	23536	24083	12041	24793	8264	25447	25446	26107	4351
23539	23538	24091	2190	24799	12399	25453	4242	26111	13055
23549	3364	24097	8032	24809	3101	25457	25456	26113	26112
23557	5889	24103	2678	24821	24820	25463	25462	26119	13059
23561	1178	24107	12053	24841	12420	25469	25468	26141	26140
23563	11781	24109	8036	24847	24846	25471	12735	26153	3736
23567	23566	24113	24112	24851	4970	25523	12761	26161	2616
23581	23580	24121	4020	24859	24858	25537	25536	26171	26170
23593	23592	24133	2011	24877	12438	25541	25540	26177	26176
23599	874	24137	24136	24889	3111	25561	6390	26183	2014
23603	11801	24151	12075	24907	12453	25577	25576	26189	26188
23609	11804	24169	3021	24917	12458	25579	8526	26203	13101
23623	7874	24179	154	24919	12459	25583	25582	26209	13104
23627	11813	24181	4836	24923	12461	25589	25588	26227	1457
23629	7876	24197	12098	24943	8314	25601	25	26237	6559
23633	23632	24203	12101	24953	24952	25603	4267	26249	6562
23663	23662	24223	24222	24967	24966	25609	6402	26251	26250

TABLE III. (*continued*).

26261	26260	26881	3360	27583	27582	28211	5642	28813	2058
26263	2918	26891	26890	27611	27610	28219	28218	28817	28816
26267	13133	26893	6723	27617	27616	28229	28228	28837	1602
26293	2191	26903	26902	27631	4605	28277	14138	28843	14421
26297	26296	26921	3365	27647	27646	28279	14139	28859	28858
26309	26308	26927	26926	27653	6913	28283	14141	28867	4811
26317	1462	26947	13473	27673	9224	28289	14144	28871	14435
26321	13160	26951	13475	27689	13844	28297	28296	28879	14439
26339	2026	26953	26952	27691	9230	28307	14153	28901	5780
26347	4391	26959	13479	27697	27696	28309	28308	28909	28908
26357	6589	26981	26980	27701	27700	28319	14159	28921	7230
26371	26370	26987	13493	27733	4622	28349	28348	28927	3214
26387	13193	26993	26992	27737	27736	28351	14175	28933	14466
26393	26392	27011	27010	27739	13869	28387	14193	28961	14480
26399	13199	27017	27016	27743	27742	28393	1352	28979	28978
26407	26406	27031	4505	27749	27748	28403	14201	29009	14504
26417	26416	27043	13521	27751	925	28409	7102	29017	29016
26423	26422	27059	27058	27763	13881	28411	28410	29021	29020
26431	2643	27061	27060	27767	27766	28429	28428	29023	29022
26437	6609	27067	347	27773	6943	28433	28432	29027	14513
26449	1653	27073	27072	27779	27778	28439	14219	29033	29032
26459	26458	27077	6769	27791	13895	28447	9482	29059	9686
26479	4413	27091	9030	27793	27792	28463	214	29063	29062
26489	1892	27103	27102	27799	13899	28477	14238	29077	7269
26497	26496	27107	13553	27803	13901	28493	7123	29101	29100
26501	26500	27109	9036	27809	6952	28499	28498	29123	14561
26513	26512	27127	27126	27817	456	28513	28512	29129	7282
26539	26538	27143	27142	27823	27822	28517	14258	29131	29130
26557	6639	27179	27178	27827	13913	28537	28536	29137	9712
26561	6640	27191	13595	27847	27846	28541	5708	29147	14573
26573	13286	27197	6799	27851	27850	28547	14273	29153	29152
26591	2659	27211	27210	27883	13941	28549	28548	29167	29166
26597	13298	27239	13619	27893	6973	28559	14279	29173	7293
26627	13313	27241	3405	27901	27900	28571	28570	29179	9726
26633	26632	27253	13626	27917	6979	28573	14286	29191	973
26641	13320	27259	3894	27919	13959	28579	28578	29201	1825
26647	8882	27271	4545	27941	5588	28591	2859	29207	29206
26669	26668	27277	2273	27943	27942	28597	14298	29209	14604
26681	1334	27281	6820	27947	13973	28603	14301	29221	29220
26683	4447	27283	13641	27953	27952	28607	28606	29231	2923
26687	26686	27299	27298	27961	20	28619	28618	29251	5850
26693	6673	27329	13664	27967	27966	28621	3180	29269	9756
26699	26698	27337	27336	27983	27982	28627	4771	29287	29286
26701	8900	27361	6840	27997	6999	28631	14315	29297	29296
26711	2671	27367	27366	28001	3500	28643	14321	29303	2254
26713	26712	27397	2283	28019	28018	28649	7162	29311	14655
26717	6679	27407	27406	28027	4671	28657	28656	29327	29326
26723	13361	27409	13704	28031	2803	28661	28660	29333	7333
26729	3341	27427	27426	28051	9350	28663	4777	29339	29338
26731	26730	27431	13715	28057	28056	28669	28668	29347	4891
26737	26736	27437	$6859=19^3$	28069	28068	28687	14343	29363	14681
26759	787	27449	6862	28081	1755	28697	28696	29383	29382
26777	26776	27457	9152	28087	9362	28703	28702	29389	29388
26783	26782	27479	13739	28097	28096	28711	14355	29399	14699
26801	3350	27481	4580	28099	9366	28723	4787	29401	4900
26813	13406	27487	27486	28109	28108	28729	14364	29411	5882
26821	8940	27509	27508	28111	937	28751	1150	29423	29422
26833	8944	27527	27526	28123	4687	28753	28752	29429	29428
26839	4473	27529	3441	28151	2815	28759	14379	29437	2453
26849	6712	27539	3934	28163	14081	28771	1370	29443	29442
26861	340	27541	1620	28181	5636	28789	9596	29453	199
26863	26862	27551	13775	28183	28182	28793	28792	29473	29472
26879	13439	27581	788	28201	7050	28807	9602	29483	14741

TABLE III. (*continued*).

29501	29500	29599	14799	29717	14858	29819	29818	29881	2988
29527	777	29611	90	29723	14861	29833	9944	29917	7479
29531	29530	29629	3292	29741	29740	29837	7459	29921	1496
29537	29536	29633	29632	29753	29752	29851	5970	29927	29926
29567	29566	29641	14820	29759	14879	29863	29862	29947	2139
29569	2464	29663	29662	29761	4960	29867	14933	29959	14979
29573	7393	29669	29668	29789	29788	29873	29872	29983	9994
29581	29580	29671	14835	29803	4967	29879	14939	29989	29988
29587	14793	29683	1649						

IV. "Research on the Smallpox of Sheep." By E. KLEIN, M.D., Assistant Professor at the Laboratory of the Brown Institution, London. Communicated by JOHN SIMON, F.R.S., D.C.L., Medical Officer of the Privy Council, &c. Received June 11, 1874.

Variola ovina, or smallpox of sheep, is a disease which, although it is not communicable to man, and possesses a specific contagium of its own, very closely resembles human smallpox, both as regards the development of the morbid process and the anatomical lesions which accompany it. This correspondence is so complete, that it cannot be doubted that the pathogeny of the two diseases is the same. The present investigation was therefore undertaken in the confidence that the application of the experimental method to the investigation of the ovine disease would not only yield results of value, as contributory to our knowledge of the infective process in general, but would throw special light on the pathology of smallpox.

The paper consists of four sections. In the first, the author gives an account of his experimental method, which consisted in communicating the disease by inoculation to a sufficient number of sheep, and in investigating anatomically (1) the pustules produced at the seat of inoculation, and (2) those constituting the general eruption. The lymph employed was obtained by the kindness of Prof. Chauveau, of Lyons, and Prof. Cohn, of Breslau.

In the second section, the organisms contained in fresh lymph, and the organic forms derived from them by cultivation, are described. The author finds that fresh lymph contains spheroidal bodies of extreme minuteness, which correspond to the micrococcus of Hallier and to the spheroids described by Cohn and Sanderson in vaccine lymph. It also contains other forms, not previously described, which in their development are in organic continuity with the micrococci.

The third section contains a complete anatomical description of the skin

of the sheep, with special reference to those particulars in which it differs from that of man.

The remainder of the paper is occupied with the investigation of the changes which occur in the integument at the seat of the inoculation, and with the anatomical characters of the secondary pustules.

The most important results are the following :—

1. The development of the primary pock may be divided into three stages, of which the first is characterized by progressive thickening of the integument over a rapidly increasing but well-defined area; the second, by the formation of vesicular cavities containing clear liquid (the "cells" of older authors) in the rete Malpighii; the third, by the impletion of these cavities with pus-corpuscles and other structures. It is to be noted that the division into stages is less marked than in human small-pox.

2. The process commences in the rete Malpighii and in the subjacent papillary layer of the corium—in the former, by the enlargement and increased distinctness of outline of the cells, and by corresponding germinative changes in their nuclei; in the latter, by the increase of size of the papillæ, and by germination of the epithelial elements of the capillary blood-vessels.

3. It is next seen that the interfascicular channels (lymphatic canaliculi) of the corium are dilated and more distinct; that the lining cells of these channels are enlarged and more easily recognized than in the natural state; and that, in the more vascular parts of the corium, the channels are more or less filled with migratory, or lymph, corpuscles. At the same time, the lymphatic vessels, of which the canaliculi are tributaries, can be readily traced, in consequence of their being distended with a material which resembles coagulated plasma.

4. About the third day after the appearance of the pock, the contents of the dilated lymphatics begin to exhibit characters which are not met with in ordinary exudative processes. These consist in the appearance, in the granular material already mentioned, of organized bodies, which neither belong to the tissue nor are referable to any anatomical type—viz. of spheroïdal, or ovoid, bodies having the characters of micrococci and of branched filaments. These last may be either sufficiently sparse to be easily distinguished from each other, or closely interlaced so as to form a felt-like mass.

5. The process, thus commenced, makes rapid progress. After one or two days, the greater number of the lymphatics of the affected part of the corium become filled with the vegetation above described; and on careful examination of the masses, it is seen that they present the characters of a mycelium, from which necklace-like terminal filaments spring, each of which breaks off, at its free end, into conidia. In most of the filaments, a jointed structure can be made out, and, in the larger ones, the

contents can be distinguished from the enclosing membrane by their yellowish-green colour.

6. At the same time that these appearances present themselves in the corium, those changes are beginning in the now much thickened rete Malpighii which are preparatory to the formation of the vesicular cavities already mentioned. By a process which the author designates horny transformation, having its seat in the epithelial cells of the middle layer of the rete Malpighii, a horny expansion, or stratum, appears, lying in a plane parallel to the surface, by which the rete Malpighii is divided into two parts, of which one is more superficial, the other deeper than the horny layer. Simultaneously with the formation of the horny layer the cells of the rete nearest the surface of the corium undergo very active germination, in consequence of which the interpapillary processes not only enlarge, but intrude in an irregular manner into the subjacent corium. At the same time, the cells immediately below the horny stratum begin to take part in the formation of the vesicular cavities, some of them enlarging into vesicles, while others become flattened and scaly, so as to form the septa by which the vesicular cavities are separated from each other.

7. The vesicles, once formed, increase in form and number. Originally separate, and containing only clear liquid, they coalesce, as they get larger, into irregular sinuses, and are then seen to contain masses of vegetation similar to those which have been already described in the lymphatic system of the corium—with this difference, that the filaments of which the masses are composed are of such extreme tenuity, and the conidia are so small and numerous, that the whole possesses the characters of zooglœa rather than of mycelium. However, the author has no doubt that these aggregations are produced in the same way as the others, viz. by the detachment of conidia from the ends of filaments. In the earlier stages of the process the cavities contain scarcely any young cells. Sooner or later, however, so much of the rete Malpighii as lies between the horny stratum and the papillæ becomes infiltrated with migratory lymph-corpuscles. The process can be plainly traced in the sections. At the period of vesiculation, *i. e.* at a time corresponding to the commencement of the development of the vesicles in the rete Malpighii, the cutis (particularly towards the periphery of the pock) is infiltrated with these bodies. No sooner has the coalescence of the vesicles made such progress as to give rise to the formation of a system of intercommunicating sinuses, than it is seen that the whole of the deep layers of the rete Malpighii become inundated (so to speak) with migratory cells, which soon find their way towards the cavities, and convert them into microscopical collections of pus-corpuscles, the formation of which is proved to be due to migration from the corium, not only by the actual observation of numerous amœboid cells *in transitu*, but by the fact that the corium itself,

before so crowded with these bodies, becomes, as the pustulation advances, entirely free from them.

8. The concluding section of the paper is occupied with the description of the secondary eruption, the anatomical characters of which very closely resemble those which have been already detailed.

V. "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—No. IV. By J. NORMAN LOCKYER, F.R.S. Received May 11, 1874.

(Abstract.)

Maps of the spectra of calcium, barium, and strontium have been constructed from photographs taken by the method described in a former communication (the third of this series). The maps comprise the portion of the spectrum extending from wave-length 3900 to wave-length 4500, and are laid before the Society as a specimen of the results obtainable by the photographic method, in the hope of securing the cooperation of other observers. The method of mapping is described in detail, and tables of wave-lengths accompany the maps. The wave-lengths assigned to the new lines must be considered only as approximations to the truth. Many of the coincidences between lines in distinct spectra recorded by former observers have been shown, by the photographic method, to be caused by the presence of one substance as an impurity in the other; but a certain number of coincidences still remain undetermined. The question of the reversal of the new lines in the solar spectrum is reserved till better photographs can be obtained.

VI. "An Account of certain Organisms occurring in the Liquor Sanguinis." By WILLIAM OSLER, M.D. Communicated by J. BURDON SANDERSON, M.D., F.R.S. Received May 6, 1874.

In many diseased conditions of the body, occasionally also in perfectly healthy individuals and in many of the lower animals, careful investigation of the blood proves that, in addition to the usual elements, there exist pale granular masses, which on closer inspection present a corpuscular appearance (Plate V. fig. 1). There are probably few observers in the habit of examining blood who have not, at some time or other, met with these structures, and have been puzzled for an explanation of their presence and nature.

In size they vary greatly, from half or quarter that of a white blood-corpuscle, to enormous masses occupying a large area of the field or even stretching completely across it. They usually assume a somewhat round or oval form, but may be elongated and narrow, or, from the existence of numerous projections, offer a very irregular outline. They have a compact solid look, and by focusing are seen to possess considerable depth; while in specimens examined without any reagents the filaments of fibrin adhere to them, and, entangled in their interior, white corpuscles are not unfrequently met with.

It is not from every mass that a judgment can be formed of their true nature, as the larger, more closely arranged ones have rather the appearance of a granular body, and it is with difficulty that the individual elements can be focused. When, however, the more loosely composed ones are chosen, their intimate composition can be studied to advantage, especially at the borders, where only a single layer of corpuscles may exist; and when examined with a high power (9 or 10 Hartnack) these corpuscles are seen to be pale round disks, devoid of granules and with well-defined contours. Some of the corpuscles generally float free in the fluid about the mass; and if they turn half over their profile view has the appearance of a sharp dark line (fig. 5, *a* & *b*). In water the individual corpuscles composing the mass swell greatly; dilute acetic acid renders them more distinct, while dilute potash solutions quickly dissolve them. Measurements give, for the large proportion of the corpuscles, a diameter ranging from one 8000th to one 10,000th of an inch; the largest are as much as one 5000th, and the smallest from one 15,000th to one 24,000th of an inch; so that they may be said to be from $\frac{1}{8}$–$\frac{1}{2}$ the size of a red corpuscle. In the blood of cats, rabbits, dogs, guineapigs, and rats the masses are to be found in variable numbers. New-born rats are specially to be recommended as objects of study, as in their blood the masses are commonly both numerous and large. They occur also in the blood of fœtal kittens.

Considering their prevalence in disease and among some of the lower animals, they have attracted but little notice, and possess a comparatively scanty literature. The late Prof. Max Schultze * was the first, as far as I can ascertain, to describe and figure the masses in question. He speaks of them as constant constituents of the blood of healthy individuals, but concludes that we know nothing of their origin or destiny, suggesting, however, at the same time that they may arise from the degeneration of granular white corpuscles. Schultze's observations were confined to the blood of healthy persons, and he seemed of the opinion that no pathological significance was to be attributed to them.

By far the most systematic account is given by Dr. Riess †, in an

* Archiv f. mik. Anat. Bd. i.

† Reichert u. Du Bois-Reymond's Archiv, 1872.

article in which he records the results of a long series of observations on their presence in various acute and chronic diseases. His investigations of the blood of patients, which were much more extensive than any I have been able to undertake, show that, in all exanthems and chronic affections of whatever sort, indeed in almost all cases attended with disturbance of function and debility, these masses are to be found. He concludes that their number is in no proportion to the severity of the disease, and that they are more numerous in the latter stages of an affection, after the acute symptoms have subsided. The former of these propositions is undoubtedly true, as I have rarely found masses larger or more abundant than I, at one time, obtained from my own blood when in a condition of perfect health. These two accounts may be said to comprise every thing of any importance that has been written concerning these bodies. The following observers refer to them cursorily:—Erb *, in a paper on the development of the red corpuscles, speaks of their presence under both healthy and diseased conditions: he had hoped, in the beginning of his research, that they might stand, as Zimmerman supposes (see below), in some connexion with the origin and development of the red corpuscles; but, as he proceeded, the fallacy of this view became evident to him. Bettelheim † seems to refer to these corpuscles when he speaks of finding in the blood of persons, healthy as well as diseased, small punctiform, or rod-shaped, corpuscles of various sizes. Christol and Kiener ‡ describe in blood small round corpuscles, whose measurements agree with the ones under consideration; and they also speak of their exhibiting slight movements. Riess §, in a criticism on a work of the next-mentioned author, again refers to these masses, and reiterates his statements concerning them. Birsch-Hirschfeld ‖ had noticed them and the similarity the corpuscles bore to micrococci, and suggests that under some conditions *Bacteria* might develop from them. Zimmerman ¶ has described corpuscular elements in the blood, which, with reference to the bodies in question, demand a notice here. He let blood flow directly into a solution of a neutral salt, and, after the subsidence of the coloured elements, examined the supernatant serum, in which he found, in extraordinary numbers, small, round, colourless corpuscles with weak contours, to which he gave the name of "elementary corpuscles." These he met with in human blood both in health and disease and in the blood of the lower animals; and he found gradations between the smaller (always colourless) forms and full-sized red corpuscles. He gives measurements (for the smaller ones, from one 1000th to one 800th of a line; the largest, one

* Virchow's Archiv, Bd. xxxiv.

† Wiener med. Presse, 1868, No. 13.

‡ Comptes Rendus, lxvii. 1054. Quoted in 'Centralblatt,' 1869, p. 96.

§ Centralblatt, 1873, No. 34.

‖ Centralblatt, 1873, No. 39.

¶ Virchow's Archiv, Bd. xviii.

500th to one 400th of a line), and speaks of them also as occurring in clumps and groups of globules. It is clear, on reading his account, that in part, at any rate, he refers to the corpuscles above described. Gradations such as he noticed between these and the coloured elements I have never met with, and undoubtedly he was dealing with the latter in a partially decolourized condition. Lostorfer's * corpuscles, which attracted such attention a few years ago from the assertion of the discoverer that they were peculiar to the blood of syphilitic patients, require for their production an artificial culture in the moist chamber extending over several days. They appear first after two or three days, or even sooner, as small bright corpuscles, partly at rest, partly in motion, which continue to increase in size, till, by the sixth or seventh day, they have attained the diameter of a red corpuscle, and may possess numerous processes or contain vacuoles in their interior. Blood from healthy individuals, as well as from diseases other than syphilis, has been shown to yield these corpuscles; and the general opinion at present held of them is that they are of an albuminoid nature.

The question at once most naturally arose, How is it possible for such masses, some measuring even one 400th of an inch, to pass through the capillaries, unless supposed to possess a degree of extensibility and elasticity such as their composition hardly warranted attributing to them? Neither Max Schultze nor Riess offer any suggestion on this point, though the latter thinks that they might, under some conditions, produce embolism.

During the examination of a portion of loose connective tissue from the back of a young rat, in a large vein which happened to be in the specimen, these same corpuscles were seen, not, however, aggregated together, but isolated and single among the blood-corpuscles (fig. 8); and repeated observations demonstrated the fact that, in a drop of blood taken from one of these young animals, the corpuscles were always to be found accumulated together; while, on the other hand, in the vessels (whether veins, arteries, or capillaries) of the same rat they were always present as separate elements, showing no tendency to adhere to one another. The masses, then, are formed at the moment of the withdrawal of the blood, from corpuscles previously circulating free in it.

To proceed now to the main subject of my communication. If a drop of blood containing these masses is mixed on a slide with an equal quantity of saline solution, $\frac{1}{2}$–$\frac{3}{4}$ per cent., or, better still, perfectly fresh serum, covered, surrounded with oil, and kept at a temperature of about 37° C., a remarkable change begins in the masses. If one of the latter is chosen for observation, and its outline carefully noted, it is seen, at first, that the edge presents a tolerably uniform appearance, a few filaments of

* Wiener med. Presse, 1872, p. 93. Wiener med. Wochenschrift, 1872, No. 8. Article in Archiv f. Dermatolog. 1872.

fibrin perhaps adhering to it, or a few small corpuscles lying free in the vicinity. These latter soon exhibit apparent Brownian movements, frequently turning half over, and showing their dark rod-like border (fig. 5, *a*, *b*). After a short time an alteration is noticed in the presence of fine projections from the margins of the mass, which may be either perfectly straight, or each may present an oval swelling at the free or attached end or else in the middle (fig. 2, *b*). It is further seen that the edges of the mass are now less dense, more loosely arranged, or, if small, it may have a radiated aspect. Sometimes, before any filaments are seen, a loosening takes place in the periphery of the mass, and among these semifree corpuscles the first development occurs. The projecting filaments above mentioned soon begin a wavy motion, and finally break off from the mass, moving away free in the fluid. This process, at first limited, soon becomes more general; the number of filaments which project from the mass increases, and they may be seen not only at the lateral borders, but also, by altering the focus, on the surface of the mass, as dark, sharply defined objects. The detachment of the filaments proceeds rapidly; and in a short time the whole area for some distance from the margins is alive with moving forms (fig. 2, *c*, and fig. 3), which spread themselves more and more peripherally as the development continues in the centre. In addition to the various filaments, swarming granules are present in abundance, and give to the circumference a cloudy aspect, making it difficult to define the individual forms. The mass has now become perceptibly smaller, more granular, its borders indistinct and merged in the swarming cloud about them; but corpuscles are still to be seen in it, as well as free in the field. A variable time is taken to arrive at this stage; usually, however, it takes place within an hour and a half, or even much less. The variety of the forms increases as the development goes on; and whereas, at first, spermatozoon-like or spindle-shaped corpuscles were almost exclusively to be seen, later more irregular forms appear, possessing two, three, or even more tail-like processes of extreme delicacy (fig. 5, *k*). The more active ones wander towards the periphery, pass out of the field, and become lost among the blood-corpuscles. The process reaches its height within 2½ hours, and from this time begins almost imperceptibly to decline; the area about the mass is less densely occupied by the moving forms, and by degrees becomes clearer, till at last, after six or seven hours (often less), scarcely an element is to be seen in the field, and a granular body, in which a few corpuscles yet exist, is all that remains of the mass. The above represents a typical development from a large mass in serum, such as that seen in fig. 3*.

We have next to study more in detail the process of development and the resulting forms. Commonly, the first appearance of activity is

* The mass from which this sketch was taken was seen in full development by several of the foreign visitors to the British Medical Association last year.

displayed by the small free corpuscles at the margins, which, previously quiescent, begin a species of jerky irregular movement, at one time with their pale disk-surfaces uppermost, at another presenting their dark linear profiles (fig. 5, *a* & *b*). Not unfrequently, some of these are seen with a larger or smaller segment of their circumference thicker and darker than the other (fig. 5, *c*).

Earliest, and perhaps the most plentiful, of the forms are those of a spermatozoon-like shape (fig. 5, *d*), attached to the mass either by the head or tail; while, simultaneously, long bow-shaped filaments appear (fig. 5, *e*), having an enlargement in the centre. Straight hair-like filaments (fig. 5, *f*) may also be seen, but they are not very numerous. The time which elapses before they begin the wavy movement is very variable, as is also the time when they break away after once beginning it. Filaments may be seen perfectly quiescent for more than half an hour before they move, and others may be observed quite as long in motion before they succeed in breaking away from the mass. Commonly it is in the smaller masses, and where the development is feeble, that filaments remain for any time adherent. The spermatozoon-like forms appear, at the head, on one view flattened and pale, on the other dark and linear (fig. 5, *d*); consequently the head is discoid, not spheroidal. The bow-shaped filaments also present a dark straight aspect when they turn over (fig. 5, *e*), and are by far the longest of the forms, some measuring as much as one 900th of an inch. Many intermediate forms between the round discoid corpuscles and those with long tails are met with in the field, and are figured at fig. 5, *g*.

Small rod-shaped forms are very numerous, most of which, however, on one aspect look corpuscular; but in others this cannot be detected, or only with the greatest difficulty; slight enlargements at each end may also be seen occasionally in these forms (fig. 5, *h*).

Usually late to appear, and more often seen in the profuse developments from large masses, are the forms with three or more tail-like processes attached to a small central body (fig. 5, *k*). Among the granules it is extremely difficult to determine accurately the number of these processes, the apparent number of which may also vary in the different positions assumed by the element. As to the ultimate destiny of the individual forms, I have not much to offer; I have watched single ones, with this view, for several consecutive hours without noticing any material alteration in them. The one represented at fig. 6 was watched for four hours, that at fig. 7 for five, and the changes sketched. The difficulty of following up individual filaments in this way is very great, not only from the ensuing weariness, but from the obstacle the red corpuscles offer to it.

With regard to the movement of the filaments, this, at first sight, bears some resemblance to that known as the Brownian, exhibited by

granules in the field, or sometimes by the red corpuscles; but an evident difference is soon noticed in the fact that, while the former (also the small corpuscles) undergo a change of place, the latter remain constant in one position or vary but little.

Movements like those of the ordinary rod-shaped *Bacteria* are not exhibited by them.

Circumstances which influence the development.—In blood, without the addition of saline solution or serum, no change takes place in the masses even after prolonged warming. A temperature of about 37° C. is necessary for the process; none occurs at the ordinary temperature, with or without the addition of fluid. Fresh serum is the medium most favourable to the process, added in quantity equal to the amount of blood. Not every mass develops when placed under conditions apparently favourable; but for this no good reason can, at present, be offered.

Fig. 8 represents the corpuscles among the red ones while in the vessel; and, as is there seen, they appear somewhat more elliptical on the profile view, and more elongated, than in blood after withdrawal, but present the same disk-like surfaces when they roll over. On adding saline solution or serum, and warming the preparation, development proceeds, but not to such an extent as from the masses. The individual corpuscles become elongated, some tailed, and they move about in the vessel. At fig. 9 they are seen in the vessel after three hours on the warm stage: the remarkable form seen at *a* was one 1300th of an inch in length, and had moved up from the opposite end of the vessel.

It must still be confessed, with Max Schultze, that we know nothing of the origin or destiny of these corpuscles; and once admit their existence as individual elements circulating in the blood, his suggestion, and Riess's assertion that the masses arise from the disintegration of white corpuscles, becomes quite untenable. We must also confess the same ignorance of the reasons of their increase in disease; nor do we know at all what influence they may exert in the course of chronic affections.

Finally, as there is no evidence that these bodies are in organic continuity with any other recognized animal or vegetable form, or possess the power of reproduction, nothing can at present be said of their nature or of their relation to *Bacteria.*

These observations were carried on in the Physiological Laboratory of University College, and my thanks are due to Prof. Sanderson and Mr. Schäfer for advice and valuable assistance.

EXPLANATION OF THE PLATE.

Plate V.

Fig. 1. Common forms of the masses from healthy blood. (Ocular 3, Objective 5.)

Fig. 2. A mass from healthy blood, in saline solution, showing stages of development: *a*, at 10 A.M.; *b*, at 10.30 A.M.; *c*, at 11 A.M. (Ocular 3, Objective 7.)

Fig. 3. Mass from blood of young rat (in serum) in full development, after two hours' warming. (Ocular 3, Objective 7.)

Fig. 4. Mass (young rat) with blood-corpuscles about it, to show the relative sizes. (Ocular 3, Objective 5.)

Fig. 5. Some of the developed forms as seen with No. 11 Hartnack. (See text.)

Fig. 6. Form watched for four hours. (Ocular 3, Objective 9.)

Fig. 7. Form watched for five hours. (Ocular 3, Objective 9.)

Fig. 8. Small vein in connective tissue from the back of a young rat, showing the corpuscles free among the red ones. (Ocular 3, Objective 7.)

Fig. 9. Small vein from the connective tissue of a rat (in serum), showing corpuscles and developed forms. (Ocular 3, Objective 9.)

VII. "On Coniferine, and its Conversion into the Aromatic Principle of Vanilla." By Ferd. Tiemann and Wilh. Haarmann. Communicated by A. W. Hofmann, LL.D., F.R.S. Received May 11, 1874.

The sap of the cambium of coniferous trees contains a beautiful crystalline glucoside, coniferine, which was discovered by Hartig and examined some years ago by Kubel, who arrived at the formula

$$C_{24}H_{32}O_{12} + 3aq.$$

A minute study of this compound leads us to represent the molecule of coniferine by the expression

$$C_{16}H_{22}O_8 + 2aq,$$

the percentages of which nearly coincide with the theoretical values of Kubel's formula.

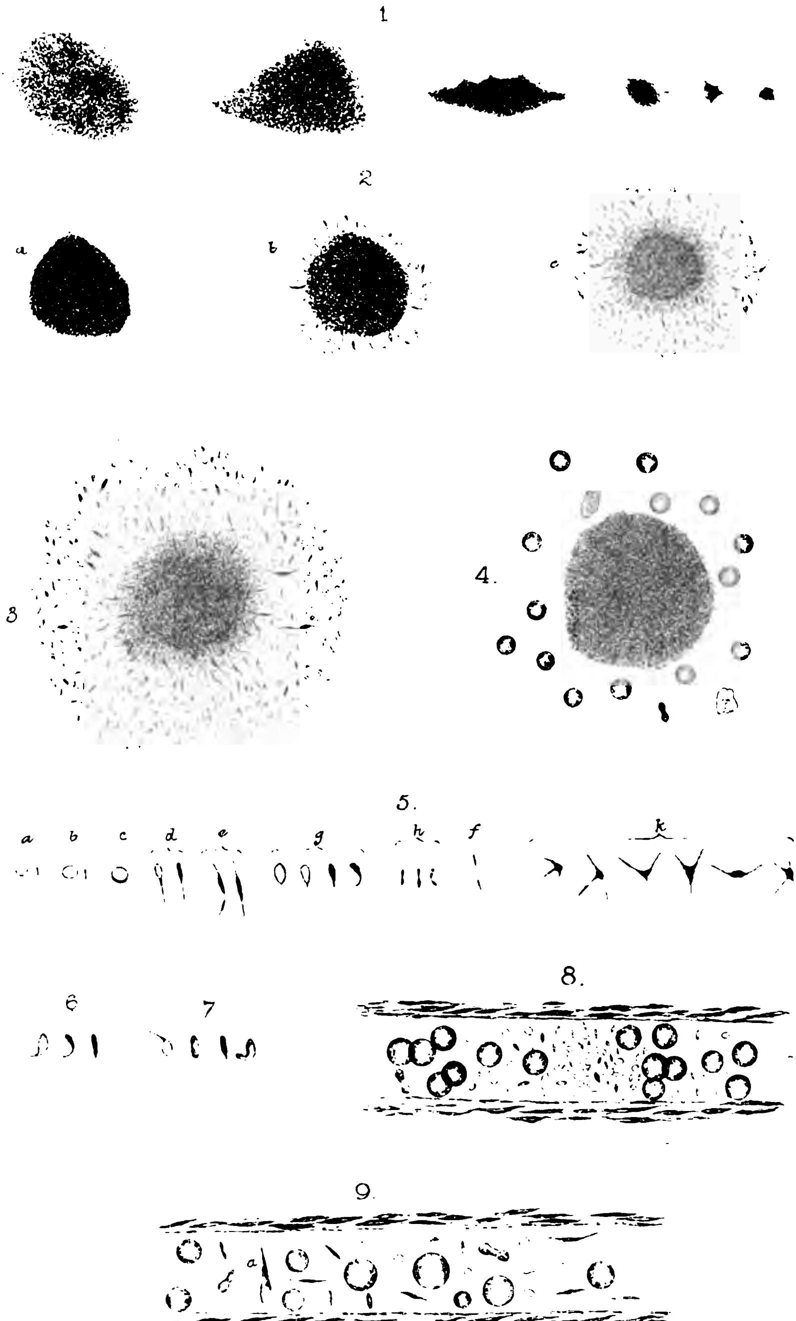
Submitted to fermentation with emulsine, coniferine splits into sugar and a splendid compound, crystallizing in prisms which fuse at 73°. This body is easily soluble in ether, less so in alcohol, almost insoluble in water; its composition is represented by the formula

$$C_{10}H_{12}O_3.$$

The change is represented by the equation

$$C_{16}H_{22}O_8 + H_2O = C_6H_{12}O_6 + C_{10}H_{12}O_3.$$

Under the influence of oxidizing agents the product of fermentation undergoes a remarkable metamorphosis. On boiling it with a mixture of potassium bichromate and sulphuric acid, there passes with the vapour of water, in the first place ethylic aldehyde, and subsequently an acid compound soluble in water, from which it may be removed by ether. On evaporating the ethereal solution, crystals in stellar groups are left behind, which fuse at 81°. These crystals have the taste and odour of vanilla. An accurate comparative examination has proved them to be iden-



W. H. Wesley Sc.

W. West & Co. imp.

tical with the crystalline substance which constitutes the aroma of vanilla, and which is often seen covering the surface of vanilla-rods.

On analysis, the crystals we obtained were found to contain

$$C_8H_8O_3.$$

This is exactly the composition which recent researches of Carles have established for the aromatic principle of vanilla. The transformation of the crystalline product of fermentation into vanilline is represented by the following equation:—

$$C_{10}H_{12}O_3 + O = C_2H_4O + C_8H_8O_3.$$

To remove all doubt regarding the identity of artificial vanilline with the natural compound, we have transformed the former into a series of salts which have the general formula

$$C_8H_7MO_3,$$

and into two substitution-products,

$$C_8H_7BrO_3$$

and

$$C_8H_7IO_3,$$

both of which had previously been prepared by Carles from the natural compound.

In order further to elucidate the nature of vanilline, we have submitted this body to fusion with alkali. The product of this action is a well-known acid discovered by Strecker, and described by him as protocatechuic acid,

$$C_7H_6O_4,$$

which is thus formed—

$$C_8H_8O_3 + 4O = C_7H_6O_4 + H_2O + CO_2.$$

We have identified this substance by analysis, by the study of its reactions, and also by transforming it into pyrocatechine, $C_6H_6O_2$,

$$C_7H_6O_4 = C_6H_6O_2 + CO_2.$$

The transformation into protocatechuic acid fixes the constitution of vanilline. This compound is the methylated aldehyde of protocatechuic acid; its composition referred to benzol is represented by the formula

$$C_6H_3\begin{cases} OCH_3 \\ OH \\ COH. \end{cases}$$

Indeed, submitted under pressure to the action of hydrochloric acid, vanilline splits into chloride of methyl and protocatechuic aldehyde,

$$C_6H_3\left\langle\begin{array}{l}OCH_3\\ OH\\ COH\end{array}\right. + HCl = CH_3Cl + C_6H_3\left\langle\begin{array}{l}OH\\ OH\\ COH.\end{array}\right.$$

A corresponding action takes place with hydriodic acid; but in this case the aldehyde is destroyed.

An additional proof of the correctness of our view regarding the constitution of vanilline is obtained by treating this substance with acetic anhydride and benzoyl chloride.

The action does not go beyond the formation of the compounds

$$C_6H_3\left\langle\begin{array}{l}OCH_3\\ OC_2H_3O\\ COH,\end{array}\right.$$

and

$$C_6H_3\left\langle\begin{array}{l}OCH_3\\ OC_7H_5O\\ COH,\end{array}\right.$$

showing that vanilline does not contain more than one hydroxylic group.

The constitution of vanilline being thus made out, there could be no doubt regarding the structure of the product of fermentation from which vanilline arises. This compound is the ethylic ether of vanilline,

$$C_6H_3\left\langle\begin{array}{l}OCH_3\\ OC_2H_5\\ COH.\end{array}\right.$$

That such is the constitution of the body is proved by the simultaneous formation of ethylic aldehyde when vanilline is formed. We obtained, however, an additional confirmation of this conception by submitting the product of fermentation to the action of hydriodic acid under pressure, when an alcohol iodide was formed, which we succeeded in separating into the iodides of methyl and ethyl,

$$C_6H_3\left\langle\begin{array}{l}OCH_3\\ OC_2H_5\\ COH\end{array}\right. + 2HI = CH_3I + C_2H_5I + C_6H_3\left\langle\begin{array}{l}OH\\ OH\\ COH.\end{array}\right.$$

The experiments we have described in this note were performed in the laboratory of Professor A. W. Hofmann, to whom we are deeply indebted for the advice and assistance he has given us in the course of these researches.

VIII. "On the Forces caused by Evaporation from, and Condensation at, a Surface." By Prof. OSBORNE REYNOLDS, of Owens College, Manchester. Communicated by B. STEWART, F.R.S. Received May 16, 1874.

It has been noticed by several philosophers, and particularly by Mr. Crookes, that, under certain circumstances, hot bodies appear to repel and cold ones to attract other bodies. It is my object in this paper to point out, and to describe experiments to prove, that these effects are the results of evaporation and condensation, and that they are valuable evidence of the truth of the kinetic theory of gas, viz. that gas consists of separate molecules moving at great velocities.

The experiments of which the explanation will be given were as follows:—

A light stem of glass, with pith-balls on its ends, was suspended by a silk thread in a glass flask, so that the balls were nearly at the same level. Some water was then put in the flask and boiled until all the air was driven out of the flask, which was then corked and allowed to cool. When cold there was a partial vacuum in it, the gauge showing from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch pressure.

It was now found that when the flame of a lamp was brought near to the flask, the pith-ball which was nearest the flame was driven away, and that with a piece of ice the pith was attracted.

This experiment was repeated under a variety of circumstances, in different flasks and with different balances, the stem being sometimes of glass and sometimes of platinum; the results, however, were the same in all cases, except such variations as I am about to describe.

The pith-balls were more sensitive to the heat and cold when the flask was cold and the tension within it low; but the effect was perceptible until the gauge showed about an inch, and even after that the ice would attract the ball.

The reason why the repulsion from heat was not apparent at greater tensions, was clearly due to the convection-currents which the heat generated within the flask. When there was enough vapour, these currents carried the pith with them; they were, in fact, then sufficient to overcome the forces which otherwise moved the pith. This was shown by the fact that when the bar was not quite level, so that one ball was higher than the other, the currents affected them in different degrees; also that a different effect could be produced by raising or lowering the position of the flame.

The condition of the pith also perceptibly affected the sensitiveness of the balls. When a piece of ice was placed against the side of the glass, the nearest of the pith-balls would be drawn towards the ice, and would eventually stop opposite to it. If allowed to remain in this condition for some time, the vapour would condense on the ball near the ice,

while the other ball would become dry (this would be seen to be the case, and was also shown, by the tipping of the balance, that ball against the ice gradually getting lower). It was then found, when the ice was removed, that the dry ball was insensible to the heat, or nearly so, while that ball which had been opposite to the ice was more than ordinarily sensitive.

If the flask were dry and the tension of the vapour reduced with the pump until the gauge showed $\frac{3}{8}$ of an inch, then, although purely steam, the vapour was not in a saturated condition, and the pith-balls which were dry were no longer sensitive to the lamp, although they would still approach the ice.

From these last two facts it appears as though a certain amount of moisture on the balls was necessary to render them sensitive to the heat.

In order that these results might be obtained, it was necessary that the vapour should be free from air. If a small quantity of air was present, although not enough to appear in the gauge, the effects rapidly diminished, particularly that of the ice, until the convection-currents had it all their own way. This agrees with the fact that the presence of a small quantity of air in steam greatly retards condensation and even evaporation.

With a dry flask and an air-vacuum, neither the lamp nor the ice produced their effects; the convection-currents reigned supreme even when the gauge was as low as $\frac{1}{4}$ inch. Under these circumstances the lamp generally attracted the balls and the ice repelled them, *i.e.* the currents carried them towards the lamp and from the ice; but, by placing the lamp or ice very low, the reverse effects could be obtained, which goes to prove that they were the effects of the currents of air.

These experiments appear to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward. These effects admit of explanation, although not quite as simply as may at first sight appear.

It seems easy to conceive that when vapour is driven off from a body there must be a certain reaction or recoil on the part of the body; Hero's engine acts on this principle. If a sheet of damp paper be held before the fire, from that side which is opposite to the fire a stream of vapour will be drawn off towards the fire with a perceptible velocity; and therefore we can readily conceive that there must be a corresponding reaction, and that the paper will be forced back with a force equal to that which urges the vapour forwards. And, in a similar way, whenever condensation goes on at a surface it must diminish the pressure at the surface, and thus draw the surface forwards.

It is not, however, wholly, or even chiefly, such visible motions as these that afford an explanation of the phenomena just described. If the only forces were those which result from the perceptible motion, they would

be insensible, except when the heat on the surface was sufficiently intense to drive the vapour off with considerable velocity. This, indeed, might be the case if vapour had no particles and was, what it appears to be, a homogeneous elastic medium, and if, in changing from liquid into gas, the expansion took place gradually, so that the only velocity acquired by the vapour was that necessary to allow its replacing that which it forces before it and giving place to that which follows.

But, although it appears to have escaped notice so far, it follows, as a direct consequence of the *kinetic* theory of gases, that, whenever evaporation takes place from the surface of a solid body or a liquid, it must be attended with a reactionary force equivalent to an increase of pressure on the surface, which force is quite independent of the perceptible motion of the vapour. Also, condensation must be attended with a force equivalent to a diminution of the gaseous pressure over the condensing surface, and likewise independent of the visible motion of the vapour. This may be shown to be the case as follows :—

According to the kinetic theory, the molecules which constitute the gas are in rapid motion, and the pressure which the gas exerts against the bounding surfaces is due to the successive impulses of these molecules, whose course directs them against the surface, from which they rebound with unimpaired velocity. According to this theory, therefore, whenever a molecule of liquid leaves the surface henceforth to become a molecule of gas, it must leave it with a velocity equal to that with which the other particles of gas rebound—that is to say, instead of being just detached and quietly passing off into the gas, it must be shot off with a velocity greater than that of a cannon-ball. Whatever may be the nature of the forces which give it the velocity, and which consume the latent heat in doing so, it is certain, from the principle of conservation of momentum, that they must react on the surface with a force equal to that exerted on the molecule, just as in a gun the pressure of the powder on the breech is the same as on the shot.

The impulse on the surface from each molecule which is driven off by evaporation must therefore be equal to that caused by the rebound of one of the reflected molecules, supposing all the molecules to be of the same size; that is to say, since the force of rebound will be equal to that of stopping, the impulse from a particle driven off by evaporation will be half the impulse received from the stopping and reflection of a particle of the gas. Thus the effect of evaporation will be to increase the number of impulses on the surface; and although each of the new impulses will only be half as effective as the ordinary ones, they will add to the pressure.

In the same way, whenever a molecule of gas comes up to a surface and, instead of rebounding, is caught and retained by the surface, and is thus condensed into a molecule of liquid, the impulse which it will thus impart to the surface will only be one half as great as if it had rebounded.

Hence condensation will reduce the magnitude of some of the impulses, and therefore will reduce the pressure on the condensing surface.

For instance, if there were two surfaces in the same vapour, one of which was dry and the other evaporating, then the pressure would be greater on the moist surface than on that which was dry. And, again, if one of the surfaces was dry and the other condensing, then the pressure would be greater on the dry surface than on that which was condensing. Hence, if the opposite sides of a pith-ball in vapour were in such different conditions, the ball would be forced towards the colder side.

These effects may be expressed more definitely as follows:—

Let v be the velocity with which the molecules of the vapour move,

p the pressure on a unit of surface,

d the weight of a unit of volume of the vapour,

w the weight of liquid evaporated or condensed in a second;

then the weight of vapour which actually strikes the unit of dry surface in a second will be

$$=\frac{dv}{6},$$

and the pressure p will be given by

$$p=2\frac{dv^2}{6g}*,$$

and f (the force arising from evaporation) will be given by

$$f=\frac{wv}{g};$$

therefore

$$f=w\sqrt{\frac{3p}{gd}}.$$

Thus we have an expression for the force in terms of the quantity of water evaporated and the ratio of the pressure to the density of the vapour; and if the heat necessary to evaporate the liquid (the latent heat) is known, we can find the force which would result from a given expenditure of heat.

Applying these results to steam, we find that, at a temperature of 60°, the evaporation of 1 lb. of water from a surface would be sufficient to maintain a force of 65 lbs. for one second.

It is also important to notice that this force will be proportional to the square root of the absolute temperature, and, consequently, will be approximately constant between temperatures of 32° and 212°.

If we take mercury instead of water, we find that the force is only 6 lbs. instead of 65 lbs.; but the latent heat of mercury is only $\frac{1}{30}$ that of water, so that the same expenditure of heat would maintain nearly three times as great a force.

It seems, therefore, that in this way we can give a satisfactory ex-

* See Maxwell, 'Theory of Heat,' p. 294.

planation of the experiments previously described. When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice.

It seems to me that the same explanation may be given of Mr. Crookes's experiments; for, although my experiments were made on water and at comparatively high pressures, they were in reality undertaken to verify the explanation as I have given it. I used water in the hope of finding (as I have found) that, in a condensable vapour, the results could be obtained with a greater density of vapour (that is to say, with a much less perfect vacuum), the effect being a consequence of the saturated condition of the vapour rather than of the perfection of the vacuum.

Mr. Crookes only obtained his results when his vacuum was nearly as perfect as the Sprengel pump would make it. Up to this point he had nothing but the inverse effects, viz. attraction with heat and repulsion with cold. About the cause of these he seems to be doubtful; but I venture to think that they may be entirely explained by the expansion of the surrounding gas or vapour, and the consequent convection-currents. It must be remembered that whenever the air about a ball is expanded, and thus rendered lighter by heat, it will exercise less supporting or floating power on the ball, which will therefore tend to sink. This tendency will be in opposition to the lifting of the ascending current, and it will depend on the shape and thickness of the ball whether it will rise or fall when in an ascending current of heated gas.

The reason why Mr. Crookes did not obtain the same results with a less perfect vacuum was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which also was not in a state of saturation. In his experiments the condensable vapour was that of mercury, or something which required a still higher temperature, and it was necessary that the vacuum should be very perfect for such vapour to be any thing like pure and in a saturated condition. As soon, however, as this state of perfection was reached, then the effects were more apparent than in the corresponding case of water. This agrees well with the explanation; for, as previously shown, the effect of mercury would, for the same quantity of heat, be three times as great as that of water; and, besides this, the perfect state of the vacuum would allow the pith (or whatever the ball might be) to move much more freely than when in the vapour of water at a considerable tension.

Of course this reasoning is not confined to mercury and water; any gas which is condensed or absorbed by the balls when cold in greater

quantities than when warm would give the same results; and, as this property appears to belong to all gases, it is only a question of bringing the vacuum to the right degree of tension.

There was one fact connected with Mr. Crookes's experiments which, independently of the previous considerations, led me to the conclusion that the result was due to the heating of the pith, and was not a direct result of the radiated heat.

In one of the experiments exhibited at the Soirée of the Royal Society, a candle was placed close to a flask containing a bar of pith suspended from the middle: at first, the only thing to notice was that the pith was oscillating considerably under the action of the candle; each end of the bar alternately approached and receded, showing that the candle exercised an influence similar to that which might have been exercised by the torsion of the thread had this been stiff. After a few minutes' observation, however, it became evident that the oscillations, instead of gradually diminishing, as one naturally expected them to do, continued; and, more than this, they actually increased, until one end of the bar passed the light, after which it seemed quieter for a little, though the oscillations again increased until it again passed the light. As a great many people and lights were moving about, it seemed possible that this might be due to external disturbance, and so its full importance did not strike me. Afterwards, however, I saw that it was only to be explained on the ground of the force being connected with the temperature of the pith. During part of its swing one end of the pith must be increasing in temperature, and during the other part it must be cooling. And it is easily seen that the ends will not be hottest when nearest the light, or coldest when furthest away; they will acquire heat for some time after they have begun to recede, and lose it after they have begun to approach. There will, in fact, be a certain lagging in the effect of the heat on the pith, like that which is apparent in the action of the sun on a comet, which causes the comet to be grandest after it has passed its perihelion. From this cause it is easy to see that the mean temperature of the ends will be greater during the time they are retiring than while approaching, and hence the driving force on that end which is leaving will, on the whole, more than balance the retarding force on that which is approaching; and the result will be an acceleration, so that the bar will swing further each time until it passes the candle, after which the hot side of the bar will be opposite to the light, and will for a time tend to counteract its effect, so that the bar will for a time be quieter. This fact is independent evidence as to the nature of the force; and although it does not show it to be evaporation, it shows that it is a force depending on the temperature of the pith, and that it is not a direct result of radiation from the candle.

Since writing the above paper, it has occurred to me that, according to the kinetic theory, a somewhat similar effect to that of evaporation must result whenever heat is communicated from a hot surface to gas.

The particles which impinge on the surface will rebound with a greater velocity than that with which they approached; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas.

And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that at which they approach.

Mathematically the result may be expressed as follows—the symbols having the same meaning as before, ϵ representing the energy communicated in the form of heat, and δv the alteration which the velocity of the molecule undergoes on impact. As before,

$$p=\frac{dv^2}{3g} \text{ or } v=\sqrt{\frac{3gp}{d}};$$

and

$$\epsilon=\frac{dv}{6}\frac{(v+\delta v)^2-v^2}{2g}=\frac{dv^2\delta v}{6g} \text{ nearly,}$$

$$f=\frac{dv}{6g}\delta v;$$

$$\therefore f=\frac{\epsilon}{v}=\epsilon\sqrt{\frac{d}{3gp}}.$$

Therefore, in the case of steam at a temperature of 60°,

$$f=\frac{\epsilon}{2000};$$

and in the case of air

$$f=\frac{\epsilon}{1400}.$$

It must be remembered that ϵ depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se* and the diffusion of the heat amongst them.

It will be much increased by convection-currents; but these will (as has been already explained), to a certain extent, produce an opposite effect. It would also seem that this action cannot have had much to do with Mr. Crookes's experiments, as one can hardly conceive that much heat could be communicated to the gas or vapour in such a perfect vacuum as that he obtained, unless, indeed, the rate of diffusion varies inversely as the density of a gas*. It will be interesting, however, to see what light experiments will throw on the question.

* June 10.—Professor Maxwell has shown that the diffusion both of heat and of the gas varies inversely as the density; therefore, excepting for convection-currents, the amount of heat communicated from a surface to a gas would be independent of the density of the gas, and hence the force f would be independent of the density; that is to say, this force would remain constant as the vacuum improved, while the convection-currents and counteracting forces would gradually diminish. It seems probable, therefore, that Mr. Crookes's results are, at least in part, due to this force.

IX. "Researches on Explosives.—Fired Gunpowder." By Capt. Noble, late Royal Artillery, F.R.S., F.R.A.S., F.C.S., and F. A. Abel, F.R.S., Treas. C.S.*

(Abstract.)

After an historical review of the investigations and theoretical views relating to the results produced upon the explosion of gunpowder, which have been published during the last 150 years, the authors proceed to describe the chief objects contemplated by their researches, which are in continuation of some commenced by Captain Noble in 1868, and described in a lecture delivered at the Royal Institution in 1871.

These objects were as follow:—

First. To ascertain the products of combustion of gunpowder, fired under circumstances similar to those which exist when it is exploded in guns or mines.

Second. To ascertain the tension of the products of combustion at the moment of explosion, and to determine the law according to which the tension varies with the gravimetric density of the powder.

Third. To ascertain whether any, and, if so, what well-defined variation in the nature or proportions of the products accompanies a change in the density or size of grains of the powder.

Fourth. To determine whether any, and, if so, what influence is exerted on the nature of the metamorphosis by the pressure under which the gunpowder is fired.

Fifth. To determine the volume of permanent gas liberated by the explosion.

Sixth. To compare the explosion of gunpowder fired in a close vessel with that of similar gunpowder when fired in the bore of a gun.

Seventh. To determine the heat generated by the combustion of gunpowder, and thence to deduce the temperature at the instant of explosion.

Eighth. To determine the work which gunpowder is capable of performing on a shot in the bore of a gun, and thence to ascertain the total theoretical work, if the bore be supposed of indefinite length.

The several methods of experiment adopted by the authors, and the most important apparatus employed in their researches, are next described in detail. The experimental operations include:—1. Measurement of pressure developed; 2. Measurement of volume of permanent gases; 3. Measurement of heat developed; 4. Collection of gases; 5. Collection of solids; 6. Analysis of the gaseous and solid products.

* We have to express our acknowledgments of the valuable assistance we have received from Mr. Charles Hutchinson in making the very laborious calculations, from Mr. George Stuart in the mechanical arrangements and in carrying out the experiments themselves, and from Dr. Kellner and Messrs. Dearing, Dodd, and Hobler in the analytical portion of these researches.

The gunpowder operated upon in the experiments includes five kinds, viz. pebble powder, rifle large-grain (cannon) powder, fine-grain powder, and rifle fine-grain powder (all of Waltham-Abbey manufacture), and also a spherical pellet powder of Spanish manufacture, specially selected for experiment as presenting considerable difference in composition from the English powders. The composition of the powders is shown in the following Table :—

TABLE I.

Results of Analysis of Gunpowders employed.

Components, per cent.		Description of Gunpowders employed in Experiments.									
		Pebble powder. Waltham Abbey.		Rifle Large-grain. Waltham Abbey.		Rifle Fine-grain. Waltham Abbey.		Fine-grain. Waltham Abbey.		Spanish Spherical Pebble powder.	
Saltpetre			74·67		74·95		75·04		73·55		75·30
Potassium sulphate			0·09		0·15		0·14		0·36		0·27
Potassium chloride		....		....		....		....			0·02
Sulphur			10·07		10·27		9·93		10·02		12·42
Charcoal	Carbon	12·12	14·22	10·86	13·52	10·67	14·09	11·36	14·59	8·65	11·34
	Hydrogen	0·42		0·42		0·52		0·49		0·38	
	Oxygen	1·45		1·99		2·66		2·57		1·68	
	Ash	0·23		0·25		0·24		0·17		0·63	
Water			0·95		1·11		0·80		1·48		0·65

The quantities of gunpowder exploded in the several operations ranged from 750 grammes to 100 grammes. The following is a description of the apparatus in which the charges were exploded :—

The apparatus consisted of a mild steel vessel, of great strength, carefully tempered in oil, in the chamber of which the charge to be exploded was placed. The main orifice of the chamber was closed by a screwed plug, called the firing-plug, fitted and ground into its place with great exactness.

In the firing-plug itself was a conical hole, stopped by a plug, also ground into its place with great accuracy, and, for purposes of insulation, covered with the finest tissue-paper. Two wires (one in the insulated cone, the other in the plug) were inserted, and joined by a very fine platinum wire passing through a small glass tube filled with mealed powder. By completing connexion with a Daniell's battery, the charge could be fired.

There were two other apertures in the chamber—one communicating with the arrangement for letting the gases escape, the other containing the crusher-apparatus for determining the tension at the moment of explosion.

The pressures actually observed with the apparatus just described

varied from over 36 tons on the square inch to about 1 ton on the square inch.

The dangerous nature of the operations of explosion, carried out on so considerable a scale as in these investigations, rendered great precautions necessary. Unless the explosion-cylinder was most perfectly closed, the violent escape of gas resulted in its immediately cutting a way out for itself, destroying the arrangement for closing the apparatus.

Special observations were made to ascertain how long a period elapsed after explosion before the non-gaseous products assumed the solid form. They appeared to do this a little within two minutes after explosion, when a charge nearly filling the vessel was used.

The method employed for collecting the gaseous products as soon as possible after the explosion presented no special feature of novelty. On opening the explosion-vessel after the gases had been allowed to escape, the solid products were found collected at the bottom, there being generally an exceedingly thin (in fact, with large charges, quite an inappreciable) deposit on the sides. The surface of the deposit was generally perfectly smooth and of a very dark grey, almost black, colour. This colour, however, was only superficial, and through the black could be perceived what was probably the real colour of the surface, a dark olive-green. The surface of the deposit, and the sides of the cylinders, had a somewhat greasy appearance, and were indeed greasy to the touch. On the smooth surface were frequently observed very minute particles, in appearance like soot, but of the greasy texture to which allusion has been made.

The removal of the deposit was generally attended with great difficulty, as it formed an exceedingly hard and compact mass, which always had to be cut out with steel chisels. Lumps would frequently break off, but a considerable portion flew off before the chisel in fine dust. In various experiments, on examining the fracture as exhibited by the lumps, the variation in physical appearance was very striking, there being marked differences in colour, and also, frequently, a marked absence of homogeneity, patches of different colours being interspersed with the more uniform shade of the fracture. There was no appearance of general crystalline structure in the deposit; but, on examination with a microscope, and sometimes with the naked eye, shining crystals of metallic lustre (sulphide of iron) were observed. On the whole, the general appearance of the deposit was attended with such considerable variations, that, for minute details, reference must be made to the account of the experiments themselves. The deposit always smelt powerfully of sulphuretted hydrogen, and, frequently, strongly of ammonia. It was always exceedingly deliquescent, and after a short exposure to the air became black on the surface, gradually passing over into an inky-looking pasty mass. As in physical appearance, so in behaviour, when removed from the cylinder, there were considerable differences between the experiments. The deposit was transferred to thoroughly dried and warm bottles, and sealed

up as rapidly as possible. In most cases, during the very short time that elapsed while the transference was being made, no apparent change took place; but, in some, a great tendency to development of heat was apparent; and in one instance, in which a portion of the deposit (exhibiting this tendency in a high degree) was kept exposed to the action of the air, the rise of temperature was so great that the paper on which it was placed became charred, and the deposit itself changed colour with great rapidity, becoming a bright orange-yellow on the surface.

This tendency to heat always disappeared when the deposit was confined in a bottle and fresh access of air excluded.

The methods employed in the analysis of the gaseous and solid products of explosion differed only in a few respects from those adopted by Bunsen and Schischkoff in their investigation of the products of explosion of powder.

As regards the proportions of total solid and gaseous products furnished by the several powders, remarkable uniformity was exhibited by the results of explosion of the same powder at different pressures, and no very considerable difference existed between the proportions furnished by the three powders chiefly used in the researches. The largest grain, or pebble powder, yielded most gas; the quantity furnished by R. L. G. powder was not greatly inferior, but was decidedly more considerable than that yielded by the smallest powder (F. G.).

The composition of the *gas* furnished by the explosion of all the English powders was throughout remarkably uniform, but presented certain apparently well-defined small variations, regulated by the pressure under which the products were developed, the chief being a steady increase in the proportion of carbonic anhydride, and decrease in that of carbonic oxide, in proportion as the pressure was increased. The composition of the *solid* products exhibited much greater variations, chiefly in regard to the state of combination in which the sulphur existed. These variations were exhibited not merely by the products obtained from the different powders, but also, and to as great an extent, by those which one and the same powder furnished at different pressures, and apparently without reference to the pressure, excepting in the case of the very lowest (the powder occupying 10 per cent. of the total space in the chamber).

The authors institute a comparison between the composition of the products of explosion obtained in their experiments and the analytical results published by Bunsen and Schischkoff and other recent experimenters, and proceed to a critical examination of the methods pursued by these for obtaining the products of the composition of gunpowder, giving reasons why the results which those methods of operation have furnished cannot be accepted as representing the changes which powder undergoes when exploded in a closed space.

The authors further proceed:—It is evident that the reactions which occur among the powder-constituents, in addition to those which result

in the development of gas, of fairly uniform composition (and very uniform as regards the proportions which it bears to the solid), from powders not differing widely in constitution from each other, are susceptible of very considerable variations, regarding the causes of which it appears only possible to form conjectures. Any attempt to express, even in a comparatively complicated chemical equation, the nature of the metamorphosis which a gunpowder of average composition may be considered to undergo when exploded in a confined space would therefore only be calculated to convey an erroneous impression as to the simplicity, or the definite nature, of the chemical results and their uniformity under different conditions, while it would, in reality, possess no important bearing upon the elucidation of the theory of explosion of gunpowder.

The extensive experiments which the Committee on Explosive Substances has instituted, with English and foreign gunpowders of very various composition, have conclusively demonstrated that the influence exerted upon the action of fired gunpowder by comparatively very considerable variations in the *constitution* of the powder (except in the case of *small* charges applied in firearms) is often very small as compared with (or even more than counterbalanced by) the modifying effects of variations in the *mechanical* and *physical** properties of the powder (*i. e.* in its density, hardness, the size and form of the grains or individual masses, &c.). Hence it is not surprising to find that a fine-grain gunpowder, which differs much more in mechanical than in chemical points from the larger powder (R. L. G.) used in these experiments, should present decided differences, not only in regard to the pressures which it develops under similar conditions, but also as regards the proportions and uniformity of the products which its explosion furnishes. On the other hand, the differences in regard to size of individual masses, and other mechanical peculiarities, between the R. L. G. and pebble powders are, comparatively, not so considerable, and are in directions much less likely to affect the results obtained by explosions in perfectly closed spaces.

Again, the analysis of solid residues furnished by various kinds of gunpowder, which presented marked dissimilarity in composition, did not establish points of difference which could be traced to any influence exerted by such variations; indeed the proportions of the several products composing residues which were furnished by one and the same powder, in

* The desirability of applying these means to effecting modifications in the action of fired gunpowder was pointed out by Colonel (now General) Boxer in a memorandum submitted to the War Office in 1859; and the first Government Committee on Gunpowder, soon afterwards appointed (of which General Boxer and Mr. Abel were members), obtained successful results, which were reported officially in 1864, by limiting the alterations in the manufacture of gunpowder intended for use in heavy guns to modifications in the form, size, density, and hardness of the individual grains or masses, the composition of the powder remaining unaltered. The Committee on Explosive Substances have adhered to this system in producing gunpowder suitable for the largest Ordnance of the present day.

distinct experiments made at varied pressures, differed, in several instances, quite as greatly as those found in some of the residues of powders which presented decided differences in composition.

Although, for the reasons already given, the authors cannot attempt to offer any thing approaching a precise expression of the chemical changes which gunpowder of average composition undergoes when exploded in a confined space, they feel warranted, by the results of their experiments, in stating, with confidence, that the chemical theory of the decomposition of gunpowder, as based upon the results of Bunsen and Schischkoff and accepted in recent text-books, is certainly as far from correctly representing the general metamorphosis of gunpowder as was the old and long-accepted theory, according to which the primary products were simply potassium sulphide, carbonic anhydride, and nitrogen. Moreover, the following broad facts regarding the products furnished by the explosion of gunpowder appear to them to have been established by the analytical results arrived at.

1. The proportion of *carbonic oxide* produced in the explosion of a gunpowder in which the saltpetre and charcoal exist in proportions calculated, according to the old theory, to produce carbonic anhydride only is much more considerable than hitherto accepted.

2. The amount of *potassium carbonate* formed, under all conditions (as regards nature of the gunpowder and pressure under which it is exploded), is very much larger than has hitherto been considered to be produced, according to the results of Bunsen and Schischkoff and more recent experimenters.

3. The *potassium sulphate* is very much smaller in amount than found by Bunsen and Schischkoff, Linck, and Karolyi, even in the highest results obtained in the authors' experiments.

4. *Potassium sulphide* is never present in very considerable amount, though, generally, in much larger proportion than found by Bunsen and Schischkoff; and there appears to be strong reason for believing that, in most instances, it exists in *large* amount as a *primary* result of the explosion of gunpowder.

5. *Potassium hyposulphite* is an important product of the decomposition of gunpowder in closed spaces, though very variable in amount. It appears probable (the reasons being fully discussed in the paper) that its production is in some measure subservient to that of the sulphide; and it may perhaps be regarded as representing, at any rate to a considerable extent, that substance in powder-residue—*i. e.* as having resulted, partially and to a variable extent, from the oxidation, by liberated oxygen, of sulphide which has been formed in the first instance.

6. The proportion of *sulphur* which does not enter into the primary reaction on the explosion of powder is very variable, being in some instances high, while, in apparently exceptional results, the whole amount of sulphur contained in the powder becomes involved in the metamor-

phosis. In the case of pebble powder, the mechanical condition (size and regularity of grain) of which is perhaps more favourable to uniformity of decomposition, under varied conditions as regards pressure, than that of the smaller powders, the amount of sulphur which remains as potassium polysulphide is very uniform, except in the products obtained at the lowest pressure; and it is noteworthy that with R. L. G. powder, under the same conditions, comparatively little sulphur escapes; while in the case of F. G. powder, under corresponding circumstances, there is no free sulphur at all.

7. But little can be said with regard to those products, gaseous and solid, which, though almost always occurring in small quantities in the products, and though apparently, in some instances, obeying certain rules with respect to the proportion in which they are formed, cannot be regarded as important results of the explosion of powder. It may, however, be remarked that the regular formation of such substances as potassium sulphocyanate and ammonium carbonate, the regular escape of hydrogen and sulphydric acid from oxidation, while oxygen is occasionally coexistent, and the frequent occurrence of appreciable proportions of potassium nitrate, indicate a complexity as well as an incompleteness in the metamorphosis. Such complexity and incompleteness are, on the one hand, a natural result of the great abruptness as well as of the comparative difficulty with which the reactions between the ingredients of the mechanical mixture take place; on the other hand, they favour the view that, even during the exceedingly brief period within which chemical activity continues, other changes may occur (in addition to the most simple, which follow immediately upon the ignition of the powder) when explosions take place at pressures such as are developed under practical conditions.

The tendency to incompleteness of metamorphosis, and also to the development of secondary reactions, under favourable conditions, appears to be fairly demonstrated by the results obtained in exploding the different powders in spaces ten times that which the charges occupied (Experiments 8, 1, and 16). It appears, however, that, even under conditions apparently the most favourable to uniformity of metamorphosis (namely, in explosions produced under high pressures), accidental circumstances may operate detrimentally to the simplicity and completeness of the reactions. But the fact, indisputably demonstrated in the course of these researches, that such accidental variations in the nature of the changes resulting from the explosion do not, even when very considerable, affect the force exerted by fired gunpowder, as demonstrated by the recorded pressures, &c., indicates that a minute examination into the nature of the products of explosion of powder does not necessarily contribute, directly, to a comprehension of the causes which may operate in modifying the action of fired gunpowder.

In illustration of the analytical results obtained in these investigations

the following statement is given of the percentage composition of the products of explosion, under one or two different pressures, of the three principal powders used.

Table II.

Showing illustrative Examples of the Analytical Results obtained.

	Pebble.		R. L. G.		F. G.	
Pressure of explosion in tons per square inch	1·4	12·5	1·6	35·6	3·7	18·2
Percentage weight of solid products	56·12	55·17	57·22	57·14	58·17	58·09
Percentage weight of gaseous products	43·88	44·83	42·78	42·86	41·83	41·92
Percentage weights of solid products of explosion:—						
Potassium carbonate	55·50	56·15	52·56	65·71	59·39	43·03
sulphate	15·02	11·93	20·47	8·52	24·22	21·00
hyposulphite	20·73	6·12	20·37	8·59	5·30	32·07
monosulphide	7·41	19·12	4·02	7·23	5·12	......
sulphocyanate	0·09	0·23	trace	0·36	0·02	0·23
nitrate	0·48	0·20	0·56	0·19	0·08	0·19
oxide	......	......	......	......	......	2·98
Ammonium sesquicarbonate	0·16	0·08	0·06	0·18	0·15	0·03
Sulphur	0·61	6·17	1·25	9·22	5·72	0·47
Carbon	trace	trace	0·71	......	trace	trace
Percentage volumes of gaseous products:—						
Carbonic anhydride	46·66	49·82	48·99	51·79	47·41	53·02
Carbonic oxide	14·76	13·36	8·98	8·32	12·35	7·91
Nitrogen	32·75	32·19	35·60	34·64	32·35	34·20
Sulphydric acid	3·13	1·96	4·06	2·61	3·76	2·03
Marsh-gas	......	0·58	0·29	0·41	......	0·50
Hydrogen	2·70	2·08	2·07	2·04	4·13	2·13
Oxygen	......	......	......	0·18	......	0·15

Table III.

Showing the composition by weight of the products of explosion of a gramme of powder as furnished by the above examples.

	Pebble.		R. L. G.		F. G.	
	grm.	grm.	grm.	grm.	grm.	grm.
Potassium carbonate	·3115	·3098	·3007	·3755	·3454	·2499
hyposulphite	·1163	·0338	·1166	·0491	·0308	·1863
sulphate	·0843	·0658	·1171	·0487	·1409	·1220
sulphide	·0416	·1055	·0230	·0413	·0298	......
sulphocyanate	·0005	·0013	·0000	·0021	·0001	·0013
nitrate	·0027	·0011	·0032	·0011	·0005	·0011
oxide	......	......	......	......	......	·0173
Ammonium sesquicarbonate	·0009	·0004	·0003	·0009	·0009	·0002
carbon	......	......	·0072	......	......	......
sulphur	·0034	·0340	·0041	·0527	·0333	·0027
Total solid	·5612	·5517	·5722	·5714	·5817	·5808
Sulphydric acid	·0134	·0084	·0166	0077	·0154	·0081
Oxygen	......	......	......	......	......	·0006
Carbonic oxide	·0519	·0473	·0303	·0356	·0416	·0258
Carbonic anhydride	·2577	·2770	·2597	·2750	·2512	·2718
Marsh-gas	......	·0012	·0006	·0015	......	·0009
Hydrogen	·0007	·0005	·0005	·0003	·0010	·0005
Nitrogen	·1151	·1139	·1201	·1085	·1091	·1117
Total gaseous	·4388	·4483	·4278	·4286	·4183	·4192

As it was one of the principal objects of the authors to determine, with as much accuracy as possible, not only *the tension of fired gunpowder* when filling completely the space in which it was exploded, but also to determine *the law according to which the tension varied with the density*, the experiments instituted to ascertain these important points were both varied and complete. The general results obtained are given in the annexed Table.

TABLE IV.

Showing the pressure corresponding to a given density of the products of explosion of F. G., R. L. G., and pebble powders, as deduced from actual observation, in a close vessel.

Mean density of products of explosion.	Corresponding pressures for pebble and R. L. G. powders.	Corresponding pressures for F. G. powder.	Mean density of products of explosion.	Corresponding pressures for pebble and R. L. G. powders.	Corresponding pressures for F. G. powder.
	Tons per square inch.	Tons per square inch.		Tons per square inch.	Tons per square inch.
·10	1·47	1·47	·60	14·39	14·02
·20	3·26	3·26	·70	19·09	18·31
·30	5·33	5·33	·80	25·03	23·71
·40	7·75	7·74	·90	32·46	30·39
·50	10·69	10·59	1·00	41·70	38·52

The *determination of the heat* developed by the explosion was also made the subject of careful direct experiment, and, from the mean of several closely concordant results, it was found that the combustion of a gramme of the powders experimented with generated about 705 gramme-units of heat. Bunsen and Schischkoff's assumption, that the specific heats of the solid products remain invariable over the great range of temperature through which they pass, is considered by the authors untenable; they have, however, deduced the temperature (about 3800° C.) upon this hypothesis, both to facilitate comparison of their results with those of Bunsen and Schischkoff, and to give a high limit, to which the temperature of explosion can certainly not attain.

The *volume of solid products* obtained from a gramme of powder is fixed by the authors at about ·3 cub. cent. at ordinary temperatures.

A comparison is next instituted of the pressures actually observed to exist in a close vessel with that calculated upon the assumption that, at the moment of explosion, about 57 per cent. by weight of the products of explosion are non-gaseous, and 43 per cent. in the form of permanent gases. The relation between the pressure and the density of the products of combustion may be expressed by the following equation,

$$p = \text{const.} \times \frac{\delta}{1 - a\delta} \quad \ldots\ldots\ldots\ldots \quad (3)$$

(a being a constant determined from the experiments); and a comparison of the results is given in the following Table:—

TABLE V.

Showing the comparison, in tons per square inch, between the pressures actually observed in a close vessel and those calculated from the formula (3).

Density of products of combustion.	Value of p deduced from direct observation.	Value of p deduced from equation (3), $a = \cdot 65$.	Density of products of combustion.	Value of p deduced from direct observation.	Value of p deduced from equation (3), $a = \cdot 65$.
	Tons per square inch.	Tons per square inch.		Tons per square inch.	Tons per square inch.
·10	1·47	1·56	·60	14·39	14·39
·20	3·26	3·36	·70	19·09	18·79
·30	5·33	5·45	·80	25·03	24·38
·40	7·75	7·91	·90	32·46	31·73
·50	10·69	10·84	1·00	41·70	41·70

The authors consider that the accordance of this comparison with observed results fully establishes the accuracy of their views.

The data furnished by the foregoing enable the authors to determine theoretically the temperature of explosion of gunpowder, which they find to be about 2200° C. The correctness of this theoretical estimate they confirm by experimental observations on the behaviour of platinum when exposed to the temperature of explosion. In all instances thin platinum wire or foil showed *signs* of fusion, but actual fusion took place only in one instance.

The *mean specific heat* of the *non-gaseous products* and their *probable expansion* between 0° C. and the temperature of explosion are next discussed.

The means of obtaining the tensions of the products of explosion in the bores of ordnance, and the results obtained in this direction by the Committee on Explosives, are then examined, as far as regards the particular powders with which the authors have experimented.

The correctness of the view propounded by Robins, that the work obtainable from gunpowder is not importantly increased by increments to the weight of the shot, is confirmed by the authors, and the influence upon the tension of fired gunpowder exerted by the existence of water in powder is illustrated.

The extent of communication of heat to the envelope (or gun) in which the powder is exploded is next considered, and experiments and calculations are given to show that such communication of heat varies from about 35 per cent. of the total heat generated in the case of a small arm to about 3 per cent. in the case of an 18-ton gun.

A comparison is instituted between the pressures actually found to exist in the bores of guns and those which would follow from the facts established by these researches. It is pointed out, on the one hand, that the assumption, that all the products of combustion are in the gaseous state, is irreconcilable with the pressures actually observed; and, on the other hand, Bunsen and Schischkoff's hypothesis that the work on the projectile is accomplished only by the permanent gases, without addition or subtraction of heat, is shown to be equally irreconcilable with

experimental observations. When, however, the heat stored up in the solid residue is taken into account, it is found that calculation and experimental observation accord with great exactness; and the authors express the relation between the tension of the products in the bore of a gun and the volume they occupy by the equation

$$p=p_0\left\{\frac{v_0(1-a)}{v-av_0}\right\}^{\frac{C_p+\beta\lambda}{C_v+\beta\lambda}} \dots\dots\dots\dots\dots\dots\dots \quad (30)^*$$

The temperature of the products of explosion during their expansion in the bore of a gun is then given, and the *maximum work* that can be realized from powder for any given expansion, as also the total *theoretic work* of powder, are given.

The principal results of the authors' investigations are summarized as follows, and for convenience are computed upon one gramme of powder, occupying 1 cub. cent.:—

(a) *First, with regard to powder fired in a close vessel.*

1. On explosion, the products of combustion consist of about 57 hundredths, by weight, of matter, which ultimately assumes the solid form, and 43 hundredths, by weight, of permanent gases.

2. At the moment of explosion, the fluid products of combustion, doubtless in a very finely divided state, occupy a volume of about ·6 cub. cent.

3. At the same instant, the permanent gases occupy a volume of ·4 cub. cent., so that both the fluid and gaseous matter are of approximately the same specific gravity.

4. The permanent gases generated by the explosion of a gramme of powder are such that, at 0° C. and 760 mm. barometric pressure, they occupy about 280 cub. cents., and therefore about 280 times the volume of the original powder.

5. The constituents of the solid products are as shown in Tables II. & III.

6. The composition of the permanent gases is shown in the same Tables.

7. The tension of the products of combustion, when the powder fills entirely the space in which it is fired, is about 6400 atmospheres, or about 42 tons per square inch.

8. The tension varies with the mean density of the products of combustion, according to the law given in equation (3).

9. About 705 gramme-units of heat are developed by the decomposition of one gramme of powder, such as used in the experiments.

10. The temperature of explosion is about 2200° C.

(b) *When powder is fired in the bore of a gun.*

1. The products of explosion, at all events as far as regards the pro-

* In this equation p denotes the tension and v the volume of the products of explosion, a the proportion occupied by the solid products, C_v and C_p the specific heats of the permanent gases at constant volume and pressure, λ the mean specific heat of the non-gaseous products, β the ratio between the weights of the gaseous and non-gaseous portions of the charge.

portions of total solid and gaseous matters, are the same as in the case of powder fired in a close vessel.

2. The work on the projectile is effected by the elastic force due to the permanent gases.

3. The reduction of temperature and pressure due to the expansion of the permanent gases is, in a great measure, compensated by the heat stored up in the liquid residue.

4. The law connecting the tension of the products of explosion with the volume they occupy is stated in equation (30).

5. The work that gunpowder is capable of performing in expanding in a vessel impervious to heat is given by the equation

$$W = \frac{p_0 v_0 (1-\alpha)(C_v+\beta\lambda)}{C_p - C_v}\left\{1-\left(\frac{v_0(1-\alpha)}{v-\alpha v_0}\right)^{\frac{C_p-C_v}{C_v+\beta\lambda}}\right\},$$

and the temperature during expansion by the equation

$$t = t_0\left(\frac{v_0(1-\alpha)}{v-\alpha v_0}\right)^{\frac{C_p-C_v}{C_v+\beta\lambda}}.$$

6. The total theoretic work of gunpowder, when indefinitely expanded, is about 332,000 gramme-metres per gramme of powder, or 486 foot-tons per lb. of powder.

With regard to one or two other points to which the authors specially directed their attention, they consider that their results warrant them in stating that:—

1. Very small-grain powders, such as F. G. and R. F. G., furnish very decidedly smaller proportions of gaseous products than a large-grain powder (R. L. G.); while the latter, again, furnishes somewhat smaller proportions than a still larger powder (pebble), though the difference between the total gaseous products of these two powders is comparatively inconsiderable.

2. The variations in the composition of the products of explosion furnished, in close chambers, by one and the same powder under *different* conditions as regards pressure, and by two powders of similar composition under the *same* conditions as regards pressure, are so considerable that no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of a gunpowder of normal composition.

3. The proportions in which the several constituents of solid powder-residue are formed are quite as much affected by slight accidental variations in the conditions which attend the explosion of one and the same powder in different experiments, as by decided differences in the composition, as well as in the size of grain, of different powders.

4. In all but very exceptional results, the solid residue furnished by the explosion of gunpowder contains, as important constituents, potassium carbonate, sulphate, hyposulphite, and sulphide, the proportion of carbonate being very much higher, and that of sulphate very much lower, than is stated by recent investigators.

X. "On the Diuretic Action of *Digitalis*." By T. LAUDER BRUNTON, M.D., D.Sc., and HENRY POWER, M.B., F.R.C.S. Communicated by Dr. SANDERSON, F.R.S. Received June 1, 1874.

It has been shown, by Max Herrmann and Ludwig, that the rapidity of the urinary secretion depends on the difference in pressure between the blood in the renal glomeruli and the urine in the urinary tubules.

At present, it is generally assumed that the diuretic action of *Digitalis* is not caused by any specific influence of the drug upon the kidney, but is due exclusively to its power of increasing the blood-pressure in the arterial system.

The results of some experiments made by us nearly a year ago show that this is not the fact. On injecting a considerable dose of digitalin (1–2 centigrammes) into the veins of an etherized dog, we have observed that the secretion of urine was either greatly diminished or ceased altogether, while the blood-pressure rose, occasionally to a considerable extent. After some time the blood-pressure again fell; and in some of the experiments the secretion of urine recommenced at the instant the fall began. In other instances it did not recommence till the blood-pressure had sunk below the normal. Occasionally the secretion did not flow with its original rapidity, but in others it was poured forth copiously, even although the blood-pressure had sunk considerably below the normal.

If *Digitalis* acted as a diuretic only by raising the blood-pressure, the flow of urine should have been greatly increased immediately after the injection, and should have diminished with the fall of arterial tension. Instead of this the secretion was least when the blood-pressure was highest, and most copious when the tension had fallen below the normal.

The explanation we would offer of these phenomena is, that *Digitalis* probably stimulates the vaso-motor nerves generally, but affects those of the kidney more powerfully than those of other parts of the body. Thus, it causes a moderate contraction of the systemic vessels, and raises the blood-pressure in them, but, at the same time, produces excessive contraction of the renal vessels, so as to stop the circulation in the kidneys and arrest the secretion of urine.

As the action of the drug on the systemic vessels passes off they relax, and the blood-pressure falls; but the renal arteries probably dilate more quickly and to a greater extent than the others. The pressure of blood in the glomeruli may thus be increased above that normally present in them, although the tension in the arterial system generally may have fallen below the normal.

Additional evidence in favour of this explanation is afforded by the fact that the urine collected after the reestablishment of secretion contains albumen, just as Herrmann found it to do after mechanical arrest of the circulation through the renal arteries.

We do not overlook the possibility that the alteration in secretion may be partly due to the direct action of the drug on the secreting elements of the kidneys, and we are still engaged in experiments on this subject.

XI. "Description of the Living and Extinct Races of Gigantic Land-Tortoises.—Parts I. and II. Introduction, and the Tortoises of the Galapagos Islands." By Dr. ALBERT GÜNTHER, F.R.S. Received June 4, 1874.

(Abstract.)

The author having had the opportunity of examining a considerable collection of the remains of Tortoises found in the islands of Mauritius and Rodriguez associated with the bones of the Dodo and Solitaire, has arrived at the following conclusions :—

1. These remains clearly indicate the former existence of several species of gigantic Land-Tortoises, the Rodriguez species differing more markedly from those of the Mauritius than these latter among themselves. All these species appear to have become extinct in modern times.

2. These extinct Tortoises of the Mascarenes are distinguished by a flat cranium, truncated beak, and a broad bridge between the foramina obturatoria.

3. All the other examples of gigantic Tortoises preserved in our museums, and said to have been brought from the Mascarenes, and likewise the single species which is known still to survive, in a wild state, in the small island of Aldabra, have a convex cranium, truncated beak, and a narrow bridge between the obturator foramina; and therefore are specifically, if not generically, distinct from the extinct ones.

4. On the other hand, there exists the greatest affinity between those contemporaries of the Dodo and Solitaire and the Tortoises still inhabiting the Galapagos archipelago.

These unexpected results induced the author to subject to a detailed examination all the available material of the gigantic Tortoises from the Mascarenes and Galapagos which are still living, or were believed to be living, and are commonly called *Testudo indica* and *Testudo elephantopus*, and to collect all the historical evidence referring to them. Thus, in the *first* (*introductory*) part of the paper a selection from the accounts of travellers is given, by which it is clearly shown that the presence of these Tortoises at two so distant stations as the Galapagos and Mascarenes cannot be accounted for by the agency of man, at least not in historical times, and therefore that these animals must be regarded as indigenous.

The *second* part consists of a description of the Galapagos Tortoises. The author shows that the opinion of some of the older travellers, viz.

that the different islands of the group are inhabited by different races, is perfectly correct; and he distinguishes four species, the adults of which are characterized as follows:—

A. *Shell* broad, with more or less corrugated plates. *Skull* with the palatal region concave; outer pterygoid edge sharp in its entire length or for the greater part of its length; a deep recess in front of the occipital condyle; anterior wall of the entrance of the tympanic cavity constricted. One of the two species is from James Island.

1. *Shell* depressed, with the upper anterior profile subhorizontal in the male, and with the striæ of the plates not deeply sculptured; sternum truncated behind. *Skull* with the facial portion very short, and with an immensely developed and raised occipital crest. *Testudo elephantopus* (Harlan).

2. *Shell* much higher, with the upper anterior profile declivous in the male, and with the striæ deeply sculptured; sternum excised behind. *Skull* with the facial portion much longer, and with low occipital crest. *Testudo nigrita* (Dum. & Bibr.).

B. *Shell* oblong, smooth. *Skull* with the palatal region shallow; the outer pterygoid edge expanded in its whole length; no deep recess in front of the occipital condyle; anterior wall of the tympanic cavity not constricted.

3. *Shell* with some traces of former concentric striæ, compressed anteriorly into the form of a "Spanish saddle" in the male; sternum truncated behind. *Skull* with the tympanic cavity much produced backwards. *Testudo ephippium* (Gthr.), from Charles Island. *Extinct.*

4. *Shell* perfectly smooth, with declivous anterior profile in the male, and with truncated posterior extremity of the sternum. *Skull* resembling that of the young of the larger species, with the tympanic case not produced backwards. The smallest species. *Testudo microphyes* (Gthr.), from Hood's Island.

Part III. will contain the account of the still existing Tortoises of the Mascarenes, and Part IV. that of the extinct species.

Received June 9, 1874.

PS. The author has just received from Professor Huxley the carapace and skeleton of another adult male, which evidently belongs to a fifth species of Galapagos Tortoises. With regard to the form of the carapace, it resembles much that of *T. elephantopus*, the dorsal shell being depressed, broad, with the upper profile nearly horizontal. Striæ distinct, broad. However, the skull differs widely from that of *T. elephantopus*, and has all the characteristics of that of *T. ephippium*, from which it differs in having a circular tympanic opening. The form of the sternum is quite peculiar, the gular portion being much constricted and produced forwards, whilst the opposite end is expanded into the large anal scutes and deeply excised. This species may be named *Testudo vicina*.

XII. "On Dredgings and Deep-sea Soundings in the South Atlantic, in a Letter to Admiral Richards, C.B., F.R.S." By Prof. Wyville Thomson, LL.D., F.R.S., Director of the Civilian Staff on board H.M.S. 'Challenger.' Received May 25, 1874.

Melbourne, March 17, 1874.

Dear Admiral Richards,—I have the pleasure of informing you that, during our voyage from the Cape of Good Hope to Australia, all the necessary observations in matters bearing upon my department have been made most successfully at nineteen principal stations, suitably distributed over the track, and including Marion Island, the neighbourhood of the Crozets, Kerguelen Island, and the Heard group.

After leaving the Cape several dredgings were taken a little to the southward, at depths from 100 to 150 fathoms. Animal life was very abundant; and the result was remarkable in this respect, that the general character of the fauna was very similar to that of the North Atlantic, many of the *species* even being identical with those on the coasts of Great Britain and Norway. The first day's dredging was in 1900 fathoms, 125 miles to the south-westward of Cape Agulhas; it was not very successful.

Marion Island was visited for a few hours, and a considerable collection of plants, including nine flowering species, was made by Mr. Moseley. These, along with collections from Kerguelen Island and from Yong Island, of the Heard group, are sent home with Mr. Moseley's notes, for Dr. Hooker's information.

A shallow-water dredging near Marion Island gave a large number of species, again representing many of the northern types, but with a mixture of southern forms, such as many of the characteristic southern Bryozoa and the curious genus *Serolis* among Crustaceans. Off Prince Edward's Island, the dredge brought up many large and striking specimens of one or two species of Alcyonarian zoophytes, allied to *Mopsea* and *Isis*.

The trawl was put down in 1375 fathoms on the 29th December, and in 1600 fathoms on the 30th, between Prince Edward's Island and the Crozets. The number of species taken in these two hauls was very large; many of them belonged to especially interesting genera, and many were new to science. I may mention that there occurred, with others, the well-known genera *Euplectella*, *Hyalonema*, *Umbellularia*, and *Flabellum*; two entirely new genera of stalked Crinoids belonging to the Apiocrinidæ; *Pourtalesia*; several Spatangoids new to science (allied to the extinct genus *Ananchytes*); *Salenia*; several remarkable Crustaceans; and a few fish.

We were unfortunately unable to land on Possession Island on account

of the weather; but we dredged in 210 fathoms and 550 fathoms, about 18 miles to the S.W. of the island, with a satisfactory result. We reached Kerguelen Island on the 7th of January, and remained there until the 1st of February. During that time Dr. v. Willemöes-Suhm was chiefly occupied in working out the land-fauna, Mr. Moseley collected the plants, Mr. Buchanan made observations on the geology of those parts of the island which we visited, and Mr. Murray and I carried on the shallow-water dredging in the steam-pinnace. Many observations were made, and large collections were stored in the different departments. We detected at Kerguelen Island some peculiarities in the reproduction of several groups of marine invertebrates, and particularly in the Echinodermata, which I have briefly described in a separate paper.

Two days before leaving Kerguelen Island, we trawled off the entrance of Christmas Harbour; and the trawl-net came up, on one occasion, nearly filled with large cup-sponges belonging to the genus *Rossella* of Carter, and probably the species dredged by Sir James Clark Ross near the ice-barrier, *Rossella antarctica*.

On the 2nd of February we dredged in 150 fathoms, 140 miles south of Kerguelen, and on the 7th of February off Yong Island, in both cases with success.

We reached Corinthian Bay, in Yong Island, on the evening of the 6th, and had made all arrangements for examining it, as far as possible, on the following day; but, to our great disappointment, a sudden change of weather obliged us to put to sea. Fortunately Mr. Moseley and Mr. Buchanan accompanied Captain Nares on shore for an hour or two on the evening of our arrival, and took the opportunity of collecting the plants and minerals within their reach. A cast of the trawl taken in lat. 60° 52′ S., long. 80° 20′ S., at 1260 fathoms, was not very productive, only a few of the ordinary deep-sea forms having been procured.

Our most southerly station was on the 14th of February, lat. 65° 42′ S., long. 79° 49′ E. The trawl brought up, from a depth of 1675 fathoms, a considerable number of animals, including Sponges, Alcyonarians, Echinids, Bryozoa, and Crustacea, all much of the usual deep-sea character, although some of the species had not been previously observed. On February 26th, in 1975 fathoms, *Umbellulariæ*, *Holothuriæ*, and many examples of several species of the *Ananchytidæ* were procured; and we found very much the same group of forms at 1900 fathoms on the 3rd of March. On the 7th of March, in 1800 fathoms, there were many animal forms, particularly some remarkable starfishes, of a large size, of the genus *Hymenaster*; and on the 13th of March, at a depth of 2600 fathoms, with a bottom-temperature of 0°·2 C., *Holothuriæ* were abundant, there were several starfishes and *Actiniæ*, and a very elegant little Brachiopod occurred attached to peculiar concretions of manganese which came up in numbers in the trawl.

In nine successful dredgings, at depths beyond 1000 fathoms, between the Cape and Australia:—

Sponges were met with on	6	occasions.
Anthozoa Octactinia	7	"
—— Polyactinia	6	"
Crinoidea	4	"
Asteroidea	8	"
Ophiuridea	9	"
Echinidea		
Holothuridea	8	"
Bryozoa	6	"
Tunicata	5	"
Sipunculacea	3	"
Nematodes	1	"
Annelida	8	"
(*Myzostomum*)	2	"
Balanoglossus	1	"
Cirripedia	4	"
Ostracoda	1	"
Isopoda	7	"
Amphipoda	3	"
Schizopoda	5	"
Decapoda Macrura	6	"
—— Brachyura	2	"
Pycnogonida	2	"
Lamellibranchiata	5	"
Brachiopoda	2	"
Gasteropoda	4	"
Cephalopoda	3	"
Teleostei	6	"

It is of course impossible to determine the species with the books of reference at our command; but many of them are new to science, and some are of great interest from their relation to groups supposed to be extinct. This is particularly the case with the Echinodermata, which are here, as in the deep water in the north, a very prominent group.

During the present cruise special attention has been paid to the nature of the bottom, and to any facts which might throw light upon the source of its materials.

This department has been chiefly in the hands of Mr. Murray; and I have pleasure in referring to the constant industry and care which he has devoted to the preparation, examination, and storing of samples. I extract from Mr. Murray's notes:—

"In the soundings about the Agulhas bank, in 100 to 150 fathoms, the bottom was of a greenish colour, and contained many crystalline par-

ticles (some dark-coloured and some clear) of Foraminifera, species of *Orbulina*, *Globigerina*, and *Pulvinulina*, a pretty species of *Uvigerina*, *Planorbulina*, *Miliolina*, *Bulimina*, and *Nummulina*. There were very few Diatoms.

"In the deep soundings and dredgings before reaching the Crozets, in 1900, 1570, and 1375 fathoms, the bottom was composed entirely of *Orbulina*, *Globigerina*, and *Pulvinulina*, the same species which we get on the surface, but all of a white colour and dead. Of Foraminifera, which we have not got on the surface, I noticed one *Rotalia* and one *Polystomella*, both dead. Some Coccoliths and Rhabdoliths were also found in the samples from these soundings. On the whole, these bottoms were, I think, the purest carbonate of lime we have ever obtained. When the soundings were placed in a bottle and shaken up with water, the whole looked like a quantity of sago. The *Pulvinulinæ* were smaller than in the dredgings in the Atlantic. We had no soundings between the Crozets and Kerguelen.

"The specimens of the bottom about Kerguelen were all from depths from 120 to 20 fathoms, and consisted usually of dark mud, with an offensive sulphurous smell. Those obtained furthest from land were made up almost entirely of matted sponge-spicules. In these soundings one species of *Rotalina* and one other Foraminifer occurred.

"At 150 fathoms, between Kerguelen and Heard Island, the bottom was composed of basaltic pebbles. The bottom at Heard Island was much the same as at Kerguelen.

"The sample obtained from a depth of 1260 fathoms, south of Heard Island, was quite different from any thing we had previously obtained. It was one mass of Diatoms, of many species; and, mixed with these, a few small *Globigerinæ* and Radiolarians, and a very few crystalline particles.

"The soundings and dredgings while we were among the ice in 1675, 1800, 1300, and 1975 fathoms, gave another totally distinct deposit of yellowish clay, with pebbles and small stones, and a considerable admixture of Diatoms, Radiolarians, and *Globigerinæ*. The clay and pebbles were evidently a sediment from the melting icebegs, and the Diatoms, Radiolarians, and Foraminifera were from the surface-waters.

"The bottom from 1950 fathoms, on our way to Australia from the Antarctic, was again exactly similar to that obtained in the 1260-fathoms sounding south of Heard Island. The bottom at 1800 fathoms, a little further to the north (lat. 50° 1′ S., long. 123° 4′ E.), was again pure '*Globigerina*-ooze,' composed of *Orbulinæ*, *Globigerinæ*, and *Pulvinulinæ*.

"The bottom at 2150 fathoms (lat. 47° 25′ S., long. 130° 32′ E.) was similar to the last, with a reddish tinge; and that at 2600 fathoms (lat. 42° 42′ S., long 134° 10′ E.) was reddish clay, the same which we got at like depths in the Atlantic, and contained manganese nodules and much decomposed Foraminifera."

Mr. Murray has been induced, by the observations which have been made in the Atlantic, to combine the use of the towing-net, at various depths from the surface to 150 fathoms, with the examination of the samples from the soundings. And this double work has led him to a conclusion (in which I am now forced entirely to concur, although it is certainly contrary to my former opinion) that the bulk of the material of the bottom in deep water is, in all cases, derived from the surface.

Mr. Murray has demonstrated the presence of *Globigerinæ*, *Pulvinulinæ*, and *Orbulinæ* throughout all the upper layers of the sea over the whole of the area where the bottom consists of "*Globigerina*-ooze" or of the red clay produced by the decomposition of the shells of Foraminifera; and their appearance when living on the surface is so totally different from that of the shells at the bottom, that it is impossible to doubt that the latter, even although they frequently contain organic matter, are all dead. I mean this to refer only to the genera mentioned above, which practically form the ooze. Many other Foraminifera undoubtedly live in comparatively small numbers, along with animals of higher groups, on the bottom.

In the extreme south the conditions were so severe as greatly to interfere with all work. We had no arrangement for heating the work-rooms; and at a temperature which averaged for some days 25° F., the instruments became so cold that it was unpleasant to handle them, and the vapour of the breath condensed and froze at once upon glass and brass work. Dredging at the considerable depths which we found near the Antarctic Circle became a severe and somewhat critical operation, the gear being stiffened and otherwise affected by the cold, and we could not repeat it often.

The evening of the 23rd of February was remarkably fine and calm, and it was arranged to dredge on the following morning. The weather changed somewhat during the night, and the wind rose. Captain Nares was, however, most anxious to carry out our object, and the dredge was put over at 5 A.M. We were surrounded by icebergs, the wind continued to rise, and a thick snow-storm came on from the south-east. After a time of some anxiety the dredge was got in all right; but, to our great disappointment, it was empty,—probably the drift of the ship and the motion had prevented its reaching the bottom. In the mean time the wind had risen to a whole gale (force=10 in the squalls), the thermometer fell to 21°·5 F., the snow drove in a dry blinding cloud of exquisite star-like crystals, which burned the skin as if they had been red-hot, and we were not sorry to be able to retire from the dredging-bridge.

Careful observations on temperature are already in your hands, reported by Captain Nares. The specific gravity of the water has been taken daily by Mr. Buchanan; and, during the trip, Mr. Buchanan has determined the amount of carbonic acid in 24 different samples—15 from the surface,

7 from the bottom, and 2 from intermediate depths. The smallest amount of carbonic acid was found in surface-water on the 27th January, near Kerguelen; it amounted to 0·0373 gramme per litre. The largest amount, 0·0829 gramme per litre, was found in bottom-water on the 14th February, when close to the Artarctic ice. About the same latitude the amount of carbonic acid in surface-water rose to the unusual amount of 0·0656 gramme per litre; in all other latitudes it ranged between 0·044 and 0·054 gramme per litre. From the greater number of these samples the oxygen and nitrogen were extracted, and sealed up in tubes.

The considerations connected with the distribution of temperature and specific gravity in these southern waters are so very complicated, that I prefer postponing any general *résumé* of the results until there has been time for full consideration.

While we were among the ice all possible observations were made on the structure and composition of icebergs. We only regretted greatly that we had no opportunity of watching their birth, or of observing the continuous ice-barrier from which most of them have the appearance of having been detached. The berg- and floe-ice was examined with the microscope, and found to contain the usual Diatoms. Careful drawings of the different forms of icebergs, of the positions which they assume in melting, and of their intimate structure were made by Mr. Wild, and instantaneous photographs of several were taken from the ship.

Upwards of 15,000 observations in meteorology have been recorded during the trip to the south. Most of these have already been tabulated and reduced to curves, and otherwise arranged for reference in considering the questions of climate on which they bear.

Many specimens in natural history have been stored in about seventy packing-cases and casks, containing, besides dried specimens, upwards of 500 store-bottles and jars of specimens in spirit.

I need only further add that, so far as I am able to judge, the expedition is fulfilling the object for which it was sent out. The naval and the civilian staff seem actuated by one wish to do the utmost in their power, and certainly a large amount of material is being accumulated.

The experiences of the last three months have of course been somewhat trying to those of us who were not accustomed to a sea-life; but the health of the whole party has been excellent. There has been so much to do that there has been little time for weariness; and the arrangements continue to work in a pleasant and satisfactory way.

(Signed) Charles Wyville Thomson.

XIII. "On the Centre of Motion in the Human Eye." By J. L. Tupper. Communicated by S. J. A. Salter, F.R.S. Received May 15, 1874.

(Abstract.)

The paper of which this is a short abstract premises that its argument is conditional, that it adopts all the fundamental optical conditions as they are received, that the received centre of motion is not one of these, but is supposed to be legitimately derived from them, and that the author disputes this and proposes :—

1st. To show that this conclusion is inconsistent with its premises, and that a different though indefinite conclusion is thence derivable;

2nd. By experiment, to develop and reduce that conclusion to a definite form;

3rd. To verify it by anatomical induction.

The latest investigations (those of Prof. Donders) have placed the centre of motion nearly two millimetres behind the centre of the globe, and in the cornea's axis. The process of proof assumed that the centre of motion is equidistant from the outer and inner margins of the cornea, and, moreover, that the eye's visual line (ordinarily at 6° with the cornea's axis) will, by mere rotation, in turn coincide with three or more radii of the same circle; or that, without moving the head, we can successively sight the lines on a graduated circular arc, seeing them as so many points.

The paper first proves, by a geometrical diagram, that if the eye, by simple rotation, can thus see the radii of a circle, the centre of motion must be in the visual line, not in the cornea's axis, as hitherto supposed; proves next, by pairs of sights set up on the radii of a circle, and actually seen as so many points, that the centre of motion *is*, in fact, in the visual line; and proves, lastly, by measuring (mechanically) how far the front of the cornea is from the converging point of the radii thus sighted, that the centre of motion is about $\frac{9}{25}$ of an inch, instead of $\frac{14}{25}$ of an inch, behind the cornea's anterior surface.

Then follows a twofold anatomical corroboration of these conclusions by examination,

1st, of the living eye;

2nd, of the dissected eye.

(1st) If the eye rotated on a point in the antero-posterior diameter (or cornea's axis), then any two points equidistant from the cornea's centre would in turn occupy the same point in space, as assumed by Prof. Donders. The first experiment shows that two such corresponding points will not, as the eye turns, fall into the same place; whilst other examinations of the living eye show not only that *symmetrically situated* points move *asymmetrically*, but move asymmetrically in such a way as would occur if the centre of motion were external to the antero-posterior axis,

or somewhere in the visual line behind the nodal point, a position which agrees with that assigned to the centre of motion by the preceding analysis.

(2nd) The dissected organ exhibits an asymmetrical attachment of the recti muscles, so that a vertical plane cutting these attachments is further from the external than from the internal margin of the cornea.

The circumference of this plane would be a circle, and the attachment of the globe's suspensory ligament, that resists the backward traction of these muscles, is found also to be a circle parallel to, and one line further back than, the former circle. The latter may be considered the base of a cone, whose vertex is the optic foramen, in the surface of which cone the recti muscles are situate. The base is therefore kept in equipoise by the symmetrical arrangement of the contracting muscles behind and the resisting suspensory ligament in front; so that the contraction of a single rectus, as it draws back the ligament on one side, increases its forward traction on the other side, and moves any two opposite points of the cone's base equally in opposite directions, or rotates it on its centre, a centre which is thus the anatomical centre of motion.

But however the recti are situate (and act) symmetrically with the base of this cone, the base is oblique with respect to the cornea (not at right angles to its axis), and consequently its centre will be on one side of the cornea's axis; and again, since the cone's base is further from the *outer* than from the *inner* margin of the cornea, its centre will be *outside* the cornea's axis. Now that part of the visual line where the preceding experiments have placed the centre of motion is *outside* the cornea's axis, while the base of the cone, whose centre has thus proved to be the anatomical centre of motion, is found to pass through the visual line $\frac{9}{25}$ of an inch behind the cornea, exactly in accordance with the results of the experiment with sighted radii of a circle.

Lastly, the obliquity of the cone's base with the base of the cornea proves to be a consequence of the hitherto unexplained want of lateral symmetry in the attachment of the recti muscles, thus explained as a most important means of adjusting the eye's visual line to the object; while some further peculiarities in the insertion of the recti, demonstrated in the author's dissections, conspire to attain the same end.

The author's thanks for valuable assistance are due to Mr. J. Salter, F.R.S., to Mr. H. G. Howse, Demonstrator of Anatomy to Guy's Hospital, and to the Rev. Geo. F. Wright, of Overslade, Rugby.

XIV. "Some Observations on Sea-water Ice." By J. Y. BUCHANAN, Chemist on Board H.M.S. 'Challenger.' Communicated by Professor A. W. WILLIAMSON, For. Sec. R.S. Received June 9, 1874.

Many different opinions have been expressed as to the nature of ice resulting from the freezing of sea-water, all agreeing, however, in one point, that, when melted, the water is unfit to drink. During the antarctic cruise of H.M.S. 'Challenger' I took an opportunity of examining some of the broken pack-ice, into which the ship made an excursion on the morning of the 25th of February, and also some ice which had formed over night in a bucket of sea-water left outside the laboratory port.

The piece of pack-ice which I examined was, in substance, clear, with many air-bells, most of them rather irregularly shaped. Two portions of this ice were allowed to melt at the temperature of the laboratory, which ranged from 2° C. to 7° C. The melting thus took place very slowly, and made it possible to examine the water fractionally. My experiments consisted in determining the chlorine in the water by means of tenth-normal nitrate-of-silver solution, and observing the temperature of the ice when melting.

A lump, which, when melted, was found to measure 625 cub. centims., was allowed to melt gradually in a porcelain dish. When about 100 cub. centims. had melted, 50 cub. centims. were taken for the determination of the chlorine; they required 13·6 cub. centims. silver solution, corresponding to 0·0483 gramme chlorine. When 560 cub. centims. had melted, 50 cub. centims. were titrated, and required 1·6 cub. centim. silver solution, corresponding to 0·0057 gramme chlorine. The remainder (65 cub. centims.) of the ice was then melted and 60 cub. centims. titrated; they required 0·39 cub. centim. silver solution, corresponding to 0·0014 gramme chlorine. We have then in the first 50 cub. centims. 0·0483 gramme chlorine, in the next 510 cub. centims. 0·0579 gramme, and in the last 65 cub. centims. 0·0015 gramme. Hence the whole lump (625 cub. centims.) contained 0·1077 gramme chlorine, or, on an average, 0·1723 gramme chlorine per litre. A qualitative analysis of the water showed lime, magnesia, and sulphuric acid to be present.

Another piece of the ice was pounded and allowed to melt in a beaker. When about half was melted, the water was poured off and found to measure 95 cub. centims.; 75 cub. centims. were titrated with silver solution, and required 1·9 cub. centim. The remainder, when melted, measured 130 cub. centims., and required 0·9 cub. centim. silver solution. Hence the first fraction of 95 cub. centims. contained 0·0085 gramme chlorine, and the second of 130 cub. centims. 0·0032 gramme chlorine. The whole quantity (225 cub. centims.) of ice therefore contained 0·0117 gramme chlorine, or, on an average, 0·0520 gramme per litre.

From these results it is evident that the ice under examination was very far from being an homogeneous body; and, indeed, nothing else could be expected, when it is borne in mind that the ice in question owes its existence, not only to the *bonâ fide* freezing of sea-water, but also to the snow which falls on its surface and is congealed into a compact mass by the salt-water spray freezing amongst it.

The ice formed by freezing sea-water in a bucket was found to have formed all round the bottom and sides of the bucket, and forming a pellicle on the surface, from which and from the sides and bottom the ice had formed in hexagonal planes, projecting edgewise into the water. The water was poured off, the crystals collected, washed with distilled water, pressed between filtering-paper, and one portion melted. It measured 9 cub. centims., and required 4 cub. centims. silver solution, corresponding to 0·0142 gramme chlorine, or 1·5780 gramme per litre. The other portion was used for determining the melting-point. The thermometer used was one of Geissler's *normal* ones, divided into tenths of a degree Centigrade, whose zero had been verified the day before in melting snow. The melting-point of the ice-crystals was found to be $-1°·3$. The temperature of the melting mass was observed to remain constant for twenty minutes, after which no further observations were made.

In the same way the melting-point of the pack-ice was determined. The fresh ice began to melt at $-1°$; after twenty minutes the thermometer had risen to $-0°·9$, and two hours and a half afterwards it stood at $-0°·3$, having remained constant for about an hour at $-0°·4$. Another portion of the ice rose more rapidly; and when three fourths of the ice was melted, the thermometer stood at 0°.

These determinations of the temperature of melting sea-water ice show that the salt is not contained in it only in the form of mechanically enclosed brine, but exists in the solid form, either as a single crystalline substance or as a mixture of ice- and salt-crystals. Common salt, when separating from solutions at temperatures below 0°, crystallizes in hexagonal planes; sea-water ice, therefore, may possibly have some analogy to the isomorphous mixtures occurring amongst minerals.

XV. "On the Physiological Action of the Chinoline and Pyridine Bases." By John G. M'Kendrick and James Dewar, Edinburgh. Communicated by Professor J. Burdon Sanderson, M.D., F.R.S. Received June 11, 1874.

(Abstract.)

It is well known that quinine, cinchonine, or strychnine yield, when distilled with caustic potash, two homologous series of bases, named the pyridine and chinoline series. Bases isomeric with these may also be

obtained by the destructive distillation of coal, or from Dippel's oil, got from bone. Greville Williams has pointed out that chinoline obtained from coal-tar differs in some respects from that yielded by cinchonine. In this research the authors endeavoured to ascertain (1) the physiological action of the various members of the series; (2) whether there was any difference in this respect between the members of the series obtained from cinchonine and those got from tar; and (3) whether, and if so, how, both as regards extent and character, the physiological action of these bases differed from that of the original alkaloidal bodies.

The bases in both series are difficult to separate from each other; but this has been done as far as possible by repeated fractional distillation. The salt employed was the hydrochlorate. This, dissolved in water, was introduced by a fine syringe under the skin of the animal. The action of chinoline was tested on frogs, mice, rabbits, guineapigs, cats, dogs, and man; but as the effects were found to be similar in all of these instances, the majority of the observations were made on rabbits. The experiments with the other substances were made on rabbits and frogs. The physiological action of hydrochlorate of chinoline was first examined. Its action was then compared with that of the hydrochlorates of the chinoline series of bases distilling at higher temperatures, including such as lepidine, dispoline, tetrahiroline, &c. In the next place, the physiological action of the pyridine series was studied, beginning with pyridine itself, and passing upwards to bases obtained at still higher boiling-points, such as picoline, lutidine, &c. Lastly, the investigation was directed to the action of condensed bases, such as dipyridine, parapicoline, &c.; and the effects of these substances were compared with those produced by the members of the chinoline series and among themselves.

The following are the general conclusions arrived at:—

1. There is a marked gradation in the extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose becomes reduced as we rise from the lower to the higher.

2. The higher members of the pyridine series resemble in physiological action the lower members of the chinoline series, except (1) that the former are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one half that of the chinolines.

3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first, as irritants, causing violent convulsions, and at length producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appear to be inactive, they may be readily roused to action by strychnine.

4. On comparing the action of such compounds as $C_9 H_7 N$ (chinoline) with $C_9 H_{13} N$ (parvoline &c.), or $C_8 H_{11} N$ (collidine) with $C_8 H_{15} N$ (conia, from hemlock), or $C_{10} H_{10} N_2$ (dipyridine) with $C_{10} H_{14} N_2$ (nicotine, from tobacco), it is to be observed that the physiological activity of the substance is, apart from chemical structure, greatest in those bases containing the larger amount of hydrogen.

5. Those artificial bases which approximate the percentage composition of natural bases are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases; but the *kind* of action is the same in both cases.

6. When the bases of the pyridine series are doubled by condensation, producing dipyridine, parapicoline, &c., they not only become more active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids having a similar chemical constitution.

7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life either by exhaustive convulsions, or by gradual paralysis of the centres of respiration, thus causing asphyxia.

8. There is no appreciable immediate action on the sympathetic system of nerves. There is probably a secondary action, because after large doses the vasomotor centre, in common with other centres, becomes involved.

9. There is no difference, so far as could be discovered, between the physiological action of bases obtained from cinchonine and those derived from tar.

XVI. "On the Calculus of Factorials." By the Rev. H. F. C. Logan, LL.D. Communicated by Professor Cayley, F.R.S. Received November 10, 1873.

(Abstract.)

Our present knowledge of what is called pure analysis has for its concrete basis the general theory of powers.

This science the author might, after Wronski, sanctioned by Lagrange, have called algorithmie, but he prefers giving it the designation *Calculus of Powers.*

The simple functions whose properties and relations it is the object of this latter calculus to determine are, first, the three direct functions or algorithms, z^n, a^z, $\sin z$; secondly, their three inverse functions or algorithms, $z^{\frac{1}{n}}$ (or $\sqrt[n]{z}$), $\log_a z$, $\sin^{-1} z$.

The author proposes to establish a new branch of analysis or algorithmie, which is based upon the general theory of factorials, and in which $z^{n/\mp\Delta z}$ replaces z^n.

The simple functions or algorithms whose properties and relations it is the province of this new calculus to determine are $z^{n/\mp\Delta z}$, $(1+h)^{\frac{z}{h}}$, $(1-h)^{\frac{z}{h}}$, $(1+h)^{-\frac{z}{h}}$, $(1-h)^{-\frac{z}{h}}$, $\sin_{-h} z$ $\sin_{+h} z$, and their inverse functions, $z^{\frac{1}{n}/\mp\Delta z}$ $\left(\text{or } \sqrt[n/\mp\frac{\Delta z}{n}]{z}\right)$, $\log_{\mp h} z$, a logarithm taken to the base $(1+h)^{\frac{1}{h}}$, or $(1-h)^{-\frac{1}{h}}$ and $\sin^{-1}_{-h} z$ $\sin^{-1}_{+h} z$.

The calculus so founded the author proposes to call the Calculus of Factorials.

The branches of the subject treated of in the present memoir will be understood from the following list of the contents of the various sections into which it is divided :—

Ch. I. § 1. Definition and properties of $z^{n/\mp\Delta z}$, or more generally $(a\pm z)^{n/\mp\Delta z}$, when n is a whole positive number.

§ 2. Factorials with a negative whole index.

§ 3. Factorials of which the index is a positive fraction.

§ 4. Factorials of which the index is a negative fraction.

§ 5. Factorial radicals.

Ch. II. § 1. Application of the theory of finite differences to factorials.

§ 2. Differenciation * of factorial exponentials and factorial logarithms.

§ 3. Development of the various simple functions into factorial series.

XVII. "On the Employment of a Planimeter to obtain Mean Values from the traces of continuously Self-recording Meteorological Instruments." By Robert H. Scott, M.A., F.R.S. Received May 23, 1874.

It is hardly necessary to remind the Fellows that the self-recording instruments employed by the Meteorological Committee at their Observatories for the continuous registration of pressure and temperature furnish their results in the form of photographic traces. The usual method of dealing with these barograms and thermograms, as they are respectively called, is to measure them at certain intervals by appropriate scales, and to treat the numerical values so obtained by arithmetical processes so as to arrive at mean results.

This method is naturally very laborious, and its accuracy is to some

* The author uses this word to denote that which in the calculus of finite differences takes the place of differentiation in the differential calculus.

extent affected by certain peculiarities found to be very commonly present in such photographic curves, and of which no satisfactory explanation has as yet been discovered. The most important of these is what is termed by us "bagging," the result of which is that the base or fiducial line of the curve is no longer a straight line, but exhibits a certain degree of curvature, so that the difficulty of determining the hourly or other values by means of an engraved scale, bearing parallel straight lines, is very considerable.

At the suggestion of Mr. Francis Galton, the Meteorological Committee gave instructions that measurements should be made of the curves by means of the instrument called Amsler's Planimeter, of which a full description, by Mr. F. J. Bramwell, F.R.S., is printed in the 'Report of the British Association' for 1872. The object of this invention is defined, in the paper quoted, to be "that the area of any figure, however irregular, can be recorded in definite standard units of measurement by the mere passage of a tracer along the perimeter of that figure."

It is perfectly obvious that the measurement of the area of the curve, if it can be executed with sufficient accuracy, must give a far more satisfactory mode of ascertaining the value of the mean ordinate of the curve than the calculation of the average of any number of measured individual ordinates, while the economy of time ensured by the use of the planimeter forms a most important recommendation for its use.

The mode of employing the instrument is as follows:—The entire perimeter of the curve, down to the base-line, is measured, and the value noted. Then *using the same base-line*, a rectangle of known height, in units of the scale of the curve, is next measured in the same way, and the value noted again.

The ratio of these two values is the mean value of the ordinate of the curve, or the mean pressure or temperature for the interval embraced by the curve.

It may be remarked that I have learnt within the last few days that the present occasion is not the first on which a planimeter has been used for the deduction of meteorological means. Mons. van Rysselberghe, Professor at the School of Navigation, Ostend, has employed it in connexion with his new electrical Meteorograph.

The subjoined Table shows for a period of eight months the means of temperature for Kew Observatory obtained by the planimeter, as well as those yielded by the old method, both for daily and for five-day means. It will be seen that the difference in 242 determinations of daily means only amounted to $0^{\circ}\cdot5$ on six occasions, and to $0^{\circ}\cdot6$ in one instance; while out of 49 cases of five-day means the greatest difference was only $0^{\circ}\cdot4$, and this was only once attained.

At the end of the Table the column headed "Wr. Rep. Plates" gives the values obtained by measurement of the plates published in the 'Quarterly Weather Report' for the period embraced by the measure-

ments to which I have just alluded. It will be seen from it that the five-day means so obtained hardly differ from those which are yielded by the direct measurement of the photographic curve by means of the planimeter.

The plates in question are obtained by the use of Mr. Francis Galton's Pantagraph, which transfers the records at a reduced time-scale to zinc plates, which plates are subsequently further reduced and transferred to copper by Wagner's Pantagraph, as explained in the Report of the Committee for 1870.

I therefore hope that the Society will allow me to remark that such a test as this affords a satisfactory proof of the accuracy of the reproductions of our automatic records which are executed in the Meteorological Office.

The result of these preliminary experiments is that the planimeter means are practically identical with those obtained by treatment of the values of the hourly ordinates.

It is found that the mean from the photographic record of temperature for one day can be obtained in about the same time as is required for the calculation of the hourly values; while in the case of pressure the saving of time would be considerable. In both cases the hourly values are supposed to have been previously measured. If, however, the five-day mean from one of the plates of the 'Quarterly Weather Report' be admissible, the economy of time would be very great indeed.

It does not appear that the liability to error in following the course of the curve with the tracer of the planimeter is greater than that of measuring the ordinates of the curve by a glass scale; while we escape one serious cause of uncertainty in the latter operation, the difficulty of assigning the exact ordinate to the hour at a period of rapid change of temperature, &c.—a case of frequent occurrence; and we almost entirely dispense with arithmetical calculations.

It is very unfortunate that the use of the planimeter will not enable us to dispense with the necessity of taking hourly readings, inasmuch as it affords us no means of averaging any but consecutive values, and so renders us no assistance in any determination of the march of meteorological phenomena.

A further series of planimeter measurements, for pressure and dry- and wet-bulb temperature, for all the observatories for three months is now in progress; and if, as we hope, the results will prove as satisfactory as those which I have the honour to submit to the Society on the present occasion, it would appear that there is no further reason why planimeter means should not be published in future by the Office.

Date.	First Day.			Second Day.			Third Day.		
Groups of Five Days.	Tabulations.	Planimeter.	Difference.	Tabulations.	Planimeter.	Difference.	Tabulations.	Planimeter.	Difference.
1872.									
April ... 1– 5.	50·2	50·4	+·2	47·1	47·6	+·5	41·3	41·7	+·4
6–10.	43·2	43·5	+·3	47·3	47·5	+·2	52·3	52·5	+·2
11–15.	53·3	53·2	−·1	56·1	56·4	+·3	51·1	51·4	+·3
16–20.	49·7	50·0	+·3	45·7	46·0	+·3	42·7	43·0	+·3
21–25.	43·5	43·5	0	47·4	47·7	+·3	49·4	49·2	−·2
26–30.	53·3	53·6	+·3	57·5	57·6	+·1	51·6	52·0	+·4
May ... 1– 5.	53·2	52·9	−·3	56·2	56·3	+·1	54·8	55·2	+·4
6–10.	47·9	48·3	+·4	50·4	50·8	+·4	47·3	47·8	+·5
11–15.	41·0	41·0	0	45·2	45·3	+·1	47·6	47·7	+·1
16–20.	53·6	53·6	0	48·9	49·2	+·3	42·0	42·3	+·3
21–25.	50·1	50·2	+·1	49·1	49·4	+·3	50·0	50·1	+·1
26–30.	56·1	56·0	−·1	61·3	60·8	−·5	61·1	61·3	+·2
31– 4.	53·0	53·4	+·4	53·4	53·2	−·2	53·4	53·6	+·2
June ... 5– 9.	56·2	56·3	+·1	54·5	55·1	+·6	50·8	50·8	0
10–14.	54·4	54·7	+·3	54·6	54·5	−·1	56·7	56·8	+·1
15–19.	64·7	64·9	+·2	68·7	68·7	0	71·4	71·1	−·3
20–24.	64·3	64·8	+·5	61·7	61·9	+·2	59·6	59·9	+·3
25–29.	61·9	62·3	+·4	57·3	57·4	+·1	57·9	57·9	0
30– 4.	60·7	60·8	+·1	61·5	61·7	+·2	62·4	62·7	+·3
July ... 5– 9.	69·5	69·8	+·3	67·7	68·0	+·3	70·0	70·0	0
10–14.	62·4	62·6	+·2	67·0	67·0	0	65·2	65·1	−·1
15–19.	58·3	58·4	+·1	62·1	62·0	−·1	59·3	59·4	+·1
20–24.	67·2	67·0	−·2	72·4	72·6	+·2	69·8	69·9	+·1
25–29.	76·1	75·9	−·2	73·2	73·2	0	68·4	68·5	+·1
30– 3.	62·0	62·2	+·2	57·6	57·7	+·1	60·8	60·8	0
August 4– 8.	58·1	58·2	+·1	59·2	59·1	−·1	61·2	61·3	+·1
9–13.	59·7	59·6	−·1	62·1	62·1	0	60·2	60·4	+·2
14–18.	59·0	59·0	0	58·7	58·8	+·1	62·9	62·9	0
19–23.	64·4	64·6	+·2	65·0	64·6	−·4	67·5	67·7	+·2
24–28.	61·4	61·3	−·1	64·8	64·8	0	61·2	61·3	+·1
29– 2.	59·9	60·1	+·2	58·5	58·7	+·2	56·7	56·7	0
Sept. ... 3– 7.	68·6	68·6	0	68·3	68·4	+·1	65·4	65·5	+·1
8–12.	60·4	60·3	−·1	61·6	61·8	+·2	59·7	59·6	−·1
13–17.	65·3	65·0	−·3	63·7	63·7	0	63·4	63·7	+·3
18–22.	57·2	57·5	+·3	52·5	52·7	+·2	46·7	46·9	+·2
23–27.	......	......	......	47·2	47·5	+·3	47·8	47·7	−·1
Oct. ... 28– 2.	......	......	......	53·3	53·6	+·3	51·4	51·3	−·1
1873.									
Jan. ... 31– 4.	35·0	35·5	+·5	30·9	31·3	+·4	29·8	30·1	+·3
Feb. ... 5– 9.	33·2	33·4	+·2	33·0	33·1	+·1	37·0	37·2	+·2
10–14.	35·8	35·9	+·1	34·0	33·9	−·1	38·2	38·4	+·2
15–19.	40·8	40·9	+·1	38·9	39·2	+·3	35·2	35·5	+·3
20–24.	32·0	32·2	+·2	30·9	31·2	+·3	37·2	37·3	+·1
25– 1.	37·5	37·4	−·1	46·3	46·5	+·2	40·3	40·6	+·3
March... 2– 6.	42·7	42·9	+·2	42·9	42·8	−·1	49·9	50·0	+·1
7–11.	44·6	44·8	+·2	42·1	42·2	+·1	44·9	45·9	+·1
12–16.	39·3	39·5	+·2	35·5	35·8	+·3	34·7	34·6	−·1
17–21.	41·3	41·5	+·2	40·5	40·6	+·1	38·9	39·0	+·1
22–26.	39·6	39·7	+·1	43·0	43·3	+·3	45·4	45·7	+·3
27–31.	44·4	44·5	+·1	45·2	45·5	+·3	47·1	47·2	+·1
	Mean Difference		+·12	Mean Difference		+·14	Mean Difference		+·14

Fourth Day.			Fifth Day.			Means of the Five Days.				
									Difference. Photo. Curve.	
Tabulations.	Planimeter.	Difference.	Tabulations.	Planimeter.	Difference.	Tabulations.	Photo. Curve.	W. Rep. Plates.	± Tabulations.	± Wr. Rep.Pls.
41·2	41·4	+·2	44·3	44·5	+·2	44·8	45·1	44·9	+·3	+·2
48·2	48·6	+·4	49·5	49·7	+·2	48·1	48·4	48·0	+·3	+·4
50·1	50·2	+·1	53·5	53·6	+·1	52·8	52·7	52·9	−·1	−·2
41·1	41·4	+·3	40·6	40·7	+·1	43·9	44·2	44·0	+·3	+·2
48·7	48·7	0	51·4	51·6	+·2	48·0	48·1	47·7	+·1	+·4
51·6	51·4	−·2	52·7	53·0	+·3	53·3	53·5	53·6	+·2	−·1
52·2	52·3	+·1	51·1	51·4	+·3	53·5	53·6	53·4	+·1	+·2
47·8	48·0	+·2	47·0	47·4	+·4	48·1	48·5	47·9	+·4	+·6
49·9	50·1	+·2	50·6	50·4	−·2	46·9	46·9	47·0	0	−·1
44·4	44·6	+·2	47·5	47·5	0	47·3	47·4	47·3	+·1	+·1
51·7	51·8	0	53·2	53·1	−·1	50·8	50·9	50·8	+·1	+·1
59·1	59·2	+·1	58·0	58·5	+·5	59·1	59·0	59·1	−·1	−·1
54·0	54·3	+·3	53·2	53·3	+·1	53·4	53·5	53·7	+·1	−·2
53·2	53·3	+·1	54·1	54·4	+·3	53·8	54·0	53·9	+·2	+·1
61·8	62·0	+·2	62·2	62·4	+·2	58·0	58·1	57·8	+·1	+·3
70·7	70·9	+·2	67·8	68·1	+·3	68·7	68·7	68·6	0	+·1
60·7	60·6	−·1	66·4	66·5	+·1	62·6	62·7	62·5	+·1	+·2
61·6	61·9	+·3	58·7	58·9	+·2	59·5	59·7	59·5	+·2	+·2
62·5	62·6	+·1	65·9	65·8	−·1	62·6	62·7	62·6	+·1	+·1
62·0	62·2	+·2	61·0	61·2	+·2	66·0	66·3	66·3	+·3	0
59·4	59·5	+·1	59·1	59·1	0	62·6	62·7	62·7	+·1	0
61·0	61·2	+·2	64·3	64·3	0	61·0	61·0	61·2	0	−·2
68·2	68·4	+·2	70·9	71·1	+·2	69·7	69·8	69·8	+·1	0
66·6	66·9	+·3	66·1	66·4	+·3	70·1	70·2	70·2	+·1	0
57·9	58·0	+·1	58·5	58·5	0	59·3	59·4	59·3	+·1	+·1
61·3	61·6	+·3	59·8	59·6	−·2	59·9	60·0	60·0	+·1	0
59·0	59·1	+·1	57·7	57·7	0	59·7	59·8	59·7	+·1	+·1
66·0	66·1	+·1	65·4	65·5	+·1	62·4	62·5	62·4	+·1	+·1
65·6	65·7	+·1	63·7	63·8	+·1	65·3	65·1	65·2	−·2	−·1
57·2	57·4	+·2	58·3	58·1	−·2	60·6	60·6	60·5	0	+·1
56·9	56·5	−·4	66·2	65·9	−·3	59·7	59·6	59·8	−·1	−·2
63·3	63·4	+·1	63·0	62·8	−·2	65·7	65·7	65·6	0	+·1
66·4	66·4	0	65·0	65·4	+·4	62·6	62·7	62·8	+·1	−·1
57·1	57·3	+·2	61·3	60·9	−·4	62·2	62·1	62·3	−·1	−·2
43·9	43·9	0	44·0	44·0	0	48·8	49·0	49·3	+·2	−·3
......	......	......	55·8	56·0	+·2	......	......	......	......	......
54·8	55·1	+·3	58·1	58·2	+·1	......	......	......	......	......
33·2	33·3	+·1	35·6	35·7	+·1	32·9	33·2	33·2	+·3	0
35·5	36·0	+·5	34·7	34·9	+·2	34·7	34·9	34·9	+·2	0
37·7	37·9	+·2	40·2	40·3	+·1	37·2	37·3	37·0	+·1	+·3
33·6	33·8	+·2	33·6	33·9	+·3	36·4	36·7	36·3	+·3	+·4
35·6	35·9	+·3	28·9	29·3	+·4	32·9	33·2	32·8	+·3	+·4
34·7	34·8	+·1	39·8	39·7	−·1	39·7	39·8	39·8	+·1	0
45·3	45·6	+·3	40·6	40·8	+·2	44·3	44·4	44·2	+·1	+·2
39·8	40·1	+·3	40·2	40·5	+·3	42·3	42·5	42·2	+·2	+·3
38·5	38·5	0	38·9	38·9	0	37·4	37·5	37·4	+·1	+·1
39·9	40·2	+·3	36·9	36·9	0	39·5	39·7	39·3	+·2	+·4
45·3	45·6	+·3	44·9	45·3	+·4	43·6	43·8	43·7	+·2	+·1
50·5	50·6	+·1	49·0	49·2	+·2	47·3	47·4	47·3	+·1	+·1
Mean Difference		+·16	Mean Difference		+·11	Mean Difference........			+·12	+·09

XVIII. "Magnetic Observations at Zi-Ka-Wei." By M. Dechevrens, Director of the Observatory. Communicated by the Rev. S. J. Perry, F.R.S. Received June 15, 1874.

Stonyhurst Observatory,
June 13th, 1874.

My dear Sir,—I enclose a report of some magnetic observations made at the New Observatory of Zi-Ka-Wei with instruments which I sent from England some time since. A complete set of self-recording magnetographs have just been completed for the same observatory by Mr. Adie, and will be forwarded to their destination this week. The Director of the Observatory of Zi-Ka-Wei, as well as his first assistant, have both received full instructions in the use of these instruments, so we may reasonably hope that the science of terrestrial magnetism will be much advanced by the foundation of this new establishment.

Yours sincerely,
S. J. Perry.

G. G. Stokes, Esq., Sec.R.S.

Premiers résultats concernant la Variation diurne de la Déclinaison à Zi-Ka-Wei (Chine).

Observations faites le 23–24 et le 29 Mars, 1874, le 6 et le 12 Avril, 1874.

Dates.	Point de départ.		Minimum.		Maximum.		Point d'arrêt.		Moyenne de la déclinaison.	Amplitude maxim. de l'excursion.	
	heure.	Déclin.	heure.	Déclin.	heure.	Déclin.	heure.	Déclin.			
Mars 23	matin. 6.30	° ′ ″ 1 55 33·8	matin. 10	° ′ ″ 1 51 4·79	soir. 2	° ′ ″ 1 58 40·19	soir. 5.30	° ′ ″ 1 54 31·79	° ′ ″ 1 54 41·29	m. s. 10–2	′ ″ 7 35·4
„ 29	6	1 56 4·19	10	1 47 19·79	2	1 55 22·79	6	1 50 46·79	1 52 23·3	m. 6–10 s. 10–2	8 44·4 8 3
Avril 6	6	1 51 33·5	9	1 49 19·3	2	1 58 20·6	5·49	1 54 32·9	1 53 28·12	-9–2	9 1·3
„ 12	5.45	1 52 27·9	8.40	1 48 46·1	2	1 54 4	5·49	1 50 51·3	1 51 32·42	8.40–2	5 17·9

[The above Table is accompanied by a figure with the results projected, and is followed by a magnetic bulletin for March 1874, from which the following mean results are extracted :—

Declination	1° 53′ 59″·8 W.
Vertical intensity	7·22996
Inclination.......................	46° 13′ 13″·7
Horizontal intensity	6·92833
Total intensity	10·0137

G. G. S.]

XIX. "Experiments with Safety-Lamps." By William Galloway, Inspector of Mines. Communicated by R. H. Scott, F.R.S. Received May 4, 1874.

After the occurrence of a great colliery-explosion it is usually very difficult, and sometimes impossible, to arrive at a satisfactory conclusion as to what were the causes which probably led to the catastrophe, and when safety-lamps have been exclusively used by the workmen its origin seems to be shrouded in mystery. The explosions which happened at Risca, Morfa, Cethin, High Brooks, and Pelton Collieries between the 1st of March, 1860, and the 21st of October, 1866, appeared to be altogether inexplicable; and, in the last two cases, when all the safety-lamps were found locked after the accident, no attempts were made to explain the phenomena.

On the 12th of December, 1866, however, the great explosion took place at the Oaks Colliery, and fortunately several of the men who survived could give an account of some of the circumstances which immediately preceded it. A stone drift had been cut from near the bottom of the downcast-shafts to within a few feet of one of the intake-airways, and shortly before the accident a shot-hole was drilled at its inner end, and charged with a considerable quantity* of gunpowder; the men who were about the pit-bottom were warned into a sheltered place; the shot was fired, and in a few seconds afterwards the shock of the explosion was felt. It was ascertained subsequently that a part of the rock at the bottom of the shot-hole had been blown into the intake-airway, leaving the tamping intact, so that the concussion of the air would be almost as great as if the tamping alone had been blown out.

A coincidence so remarkable as this attracted considerable attention, and after every great explosion which has happened since the Oaks' a search has evidently been made for some evidence of recent shot-firing. The following Table will give an idea of the magnitude of the important explosions which have happened within recent years, and of some of the circumstances under which they occurred.

Synopsis of great explosions since 1860.

Date of Explosion.	Name of Colliery.	Number of men killed.	Remarks.
1860.			
March 1	Burradon	76	Naked lights. Deficient ventilation.
December 1	Risca	142	Safety-lamps. Several explosions simultaneous with the principal one?
1862.			
February 19	Cethin	47	Naked lights and safety-lamps. Deficient ventilation.
1863.			
March 6	Coxlodge	26	Naked lights and safety-lamps. Gas from the goaves came upon the naked lights.

* $1\frac{1}{2}$ to 6 lbs. Reports of the Inspectors of Mines for the year 1866, p. 43.

Date of Explosion.	Name of Colliery.	Number of men killed.	Remarks.
1863. October 17......	Morfa..............	39	Safety-lamps; all were found in good order. Cause unknown.
1865. June 16.........	Bedwelty	26	Lamps? Gas accumulated in an unventilated heading.
December 20...	Cethin	34	Locked safety-lamps; shot-firing carried on. Cause unknown.
1866. January 23 ...	High Brooks......	30	Locked safety-lamps; all were found locked. Cause unknown.
June 14.........	Dukinfield	38	Naked lights and safety-lamps. Deficient ventilation.
October 21......	Pelton	24	Locked safety-lamps; all were found locked; shot-firing carried on. Cause unknown.
December 12...	Oaks	334	Safety-lamps. A heavily charged shot was fired in pure air a few seconds before the explosion.
December 13...	Talk o' th' Hill...	91	Safety-lamps; shot-firing carried on. Deficient ventilation.
1867. August 20......	Garswood Park...	14	Safety-lamps. A shot had blown out the tamping.
November 8 ...	Ferndale	178	Safety-lamps; shot-firing carried on. Two distinct explosions took place simultaneously in districts communicating only by two passages, and ventilated by different air-currents.
1868. November 25...	Hindley Green ...	62	Safety-lamps. A shot had blown out the tamping.
December 26...	Haydock	26	Safety-lamps. A shot had blown out the tamping.
1869. April 1	High Brooks......	37	Safety-lamps. A shot had blown out the tamping.
June 10.........	Ferndale	53	Cause unknown.
July 21	Haydock	59	Safety-lamps? An empty shot-hole was found from which it was supposed the tamping had been blown; two or more explosions took place simultaneously in distant parts of the mine.
November 15...	Low Hall	27	Safety-lamps. A shot had blown out the tamping; there appear to have been two simultaneous and very violent explosions.
1870. February 4 ...	Pendleton	9	Safety-lamps? A shot had blown out the tamping.
February 14...	Morfa..............	30	Gas from a barred-off goaf ignited at a naked light.
July 7	Silverdale	19	Lamps? Cause unknown.
July 23........	Charles Pit	19	Naked lights.
August 19......	Brynn Hall	20	Safety-lamps? A shot had blown out the tamping.
1871. January 10 ...	Renishaw Park...	26	Safety-lamps. A shot was fired with an overcharge of gunpowder; two explosions?
February 2 ...	Pentre	38	Locked safety-lamps; shot-firing carried on. A blower is supposed to have made the return air so explosive that it ignited at the ventilating-furnace.
March 2	Victoria Pit, Monmouth.	19	Gas in a stall worked with a safety-lamp; it is assumed that a naked light was carried into it.

Date of Explosion.	Name of Colliery.	Number of men killed.	Remarks.
1871.			
September 6...	Moss Pits	70	Safety-lamps. An empty shot-hole discovered after the pits were reopened. Cause unknown.
October 25......	Seaham	26	Safety-lamps. A shot was fired in pure air: one explosion of firedamp was simultaneous with the shot; another followed after a short interval?
1872.			
February 11 ...	Oakwood	11	Safety-lamps. A shot was fired.
March 28	Lover's Lane......	27	Safety-lamps? A shot had blown out the tamping.
October 7	Morley Main......	34	Safety-lamps; shot-firing carried on. Cause unknown.

It will be seen from the data given above that shot-firing was carried on in 17 of the 22 collieries at which important explosions took place after the 12th of December, 1866; safety-lamps were certainly used in 12, and probably also in the 5 which are marked doubtful; in 8 cases it was ascertained that a shot had blown out the tamping at or about the time of the explosion; in 2 an empty shot-hole was found, from which the tamping is supposed to have been blown; and in 3 a shot had been fired bringing down the coal or rock; finally, at Risca, Ferndale (1867), Haydock (1869), Low Hall, Renishaw Park, and Seaham, two or more explosions appear to have taken place simultaneously in different parts of the mine unconnected by a train of explosive gas. The Seaham explosion is a remarkable one: a heavily charged shot was fired in pure air in one of the intake-aircourses, and, according to the statement of three men who survived, the explosion of firedamp followed the shot immediately; one of the men further asserted that, in several minutes more, he heard the distinct report of another explosion.

Two methods of accounting for the simultaneousness of the explosion of firedamp with the firing of the shot have been suggested in the Reports of the Inspectors of Mines: one of them supposes that the firedamp is ignited directly by the shot; the other, that the concussion of the air caused by the explosion of gunpowder dislodges gas from cavities in the roof and from goaves, and that this gas, passing along in the air-currents, is ignited at the lamps of the workmen. In some instances, when it has been known to be highly improbable that any gas existed nearer to the shot-hole than 10, 20, or even 40 feet, the advocates of the former hypothesis have taken it for granted that the gases issuing from the shot-hole were projected through the air as far as the accumulation of firedamp, retaining a sufficiently high temperature to ignite it on their arrival. On the other hand, the advocates of the latter hypothesis have not attempted to show how the gas, which they assumed could be dislodged in quantity by a sound-wave and its reflections, could be ignited

in those cases in which safety-lamps only were used. It is no doubt highly probable, however, that when once an explosion of firedamp has been initiated in one way or another, and large bodies of air are driven through the passages of a mine with great velocity, explosive accumulations will be dislodged from cavities and goaves, and pressed through the safety-lamps with the velocity requisite to pass the flame.

In the beginning of the year 1872, when I was giving attention to this subject, it appeared to me to be probable that the sound-wave originated by a blown-out shot, in passing through a safety-lamp burning in an explosive mixture, would carry the flame through the meshes of the wire gauze, in virtue of the vibration of the molecules of the explosive gas. It had long been known*, indeed, that if an explosive current were made to impinge upon a lighted safety-lamp in a direction perpendicular to its axis, and with a velocity of 8 to 14 feet per second, the flame would pass through the meshes after a short time, and ignite the explosive mixture on the outside; but it does not seem to have been suspected that the same result might be produced by the passage of an intense sound-wave through a safety-lamp burning quietly in an explosive mixture. The explosion at Cethin Colliery in 1865 is a good example of one that may have been caused in this way, by the firing of a shot. Several days after the explosion the safety-lamp of the overman was found, securely locked and uninjured, lying at the distance of a few yards, within an abandoned stall which was known to have contained firedamp: shot-firing was carried on in this mine, and it is not improbable that a sound-wave from an overcharged or blown-out shot had passed through this lamp and ignited the explosive mixture shortly after the overman had entered it: moreover the Inspector of Mines says † he has no hesitation in stating that, in his opinion, the gas in this stall had been ignited, and was therefore the origin of the explosion; but he is unable to state by what means it was fired.

It is certain that, in every fiery mine, safety-lamps are placed in an explosive mixture from time to time, either by accident (as when men retire hurriedly, perhaps into disused places, after the fuse of a shot has been ignited) or by design to test the quality of the air, as the overman at Cethin Colliery may have been doing; and it is equally certain that shots are fired, occasionally, which blow out the tamping and cause a violent concussion of the air. If, therefore, the explanation which is brought forward in this paper to account for the relation between explosions and shot-firing be the true one, then the question as to how often explosions of this kind are likely to occur would resolve itself into one of probability as to how often an ordinary Davy or Clanny lamp, burning in an explosive mixture, would be traversed by a sound-wave of a certain amplitude of vibration.

* Transactions of North of England Institute of Mining Engineers, vols. i. & xvii.
† Reports of the Inspectors of Mines, 1865, p. 118.

On the 16th of January, 1872, I made the first experiment in connexion with this subject in the Physical Laboratory of University College, London: Professor G. C. Foster was present and co-operated with me. A sheet of wire gauze, 1 foot square, was inclined at an angle of 70°, and a slow current of gas and air from a Bunsen burner was directed against its under surface. Part of the explosive mixture thus formed passed through the meshes, and, when ignited, produced a flat flame on the upper surface 3 in. long by 1 in. wide, and symmetrically situated in regard to the sides of the sheet. A glass tube, 3 ft. 4 in. long by $3\frac{1}{2}$ in. diameter, was placed with one end at a distance of $1\frac{1}{2}$ in. from the upper surface of the sheet of wire gauze; its axis was horizontal, passed through the middle point of the flat flame, and was at right angles to the line of intersection of a horizontal plane with the sheet. At the end of the tube furthest from the wire gauze, a vessel, $3\frac{1}{4}$ in. diameter, containing a solution of soap in water was placed; the point at which the axis of the tube cut the perpendicular from the centre of the liquid was $2\frac{1}{2}$ in. from the end of the tube, and at the same distance above the surface of the liquid. An explosive mixture of coal-gas and oxygen was forced into the solution of soap until bubbles containing about 2 cub. inches had formed on the surface. A light was then applied to the gas at the upper surface of the wire gauze, and immediately afterwards to the bubbles; and after the explosion it was found that the flame had vanished from the upper surface, and that the gas issuing from the Bunsen burner was on fire.

In December 1872, I made a number of experiments similar to the foregoing in the Laboratory of the Royal College of Chemistry, when I was much indebted to Dr. Frankland for his valuable suggestions. The glass tube of the first experiment was replaced by two tin-plate tubes, each 2 in. diameter (one 10 ft. 11 in., the other 9 ft. 7 in. long); they were joined to form a continuous tube 20 ft. 6 in. long. The vessel containing the solution of soap was small enough to be placed just inside of one end of the tube, and the sheet of wire gauze was at a distance of 1 in. from the other end. The same explosive mixtures were again employed, and the same result was obtained as before. A diaphragm, consisting of four sheets of brown paper of ordinary thickness, was now inserted at the junction of the two tubes; the centre of the diaphragm was bulged to a distance of about half an inch towards the origin of disturbance. After the passage of the sound-wave, it was found that the flame had shifted to the opposite side of the wire gauze, and the diaphragm was bulged to about the same extent, but in the opposite direction. A quantity of loose cotton-wool, sufficient to fill the end of the tube completely for a length of three inches, was then pushed into the end of the one furthest from the wire gauze, at its junction with the other. After the sound-wave had passed, the flame was again found to have removed to the opposite side of the wire gauze, and the

cylinder of cotton-wool was one inch further from the origin of disturbance.

Two sets of apparatus (figs. 1 & 2, Plate VI.) were now constructed: in both the sound-wave of a pistol-shot is conveyed through tin-plate tubes to a distance of 20 feet; then it passes through a safety-lamp, which can be surrounded by an explosive mixture of gas and air.

In the apparatus represented in fig. 1 there are two tubes, each 10 feet long by 3 inches in diameter. At the end *o* a disk of wood, $\frac{3}{4}$ in. thick, with a hole in the centre large enough to receive the muzzle of the pistol, is fitted into the tube *a*; at *c* a sheet of india-rubber, $\frac{1}{16}$ in. thick, is tied over the end of the tube *b*, and a tubular ring, one end of which is covered with a network of wire $\frac{1}{16}$ in. thick, with meshes $\frac{3}{16}$ in. square, is drawn over the fastening till the network is close to the diaphragm. The part of the apparatus which is surmounted by the safety-lamp is of the following construction:—A round sheet-iron plate, *g*, of 6 in. diameter, rests on four short legs: above this, and joined to it, is a circular chamber, *f*, formed of two concentric tubular rings and two flat rings; its exterior diameter is $2\frac{1}{4}$ in., its interior diameter is $1\frac{3}{4}$ in., and in the top ring there are twenty-four small equidistant holes, whose locus is a circle of 2 in. diameter. The screw which receives the lower ring of the wire gauze is carried upon projections inwards from the upper flat ring of the chamber *f*. The wire gauze of an ordinary Davy lamp, held between two rings in the usual manner, incloses a space in which a small gas-jet occupies the position of the wick in the oil-lamp, when screwed into its place above the chamber *f*: the three stout wires joining the upper and lower rings are omitted in the figure. A rod, *l*, screwed into the plate *g*, carries a short narrow plate at its top, bent to the curve of the tube *b* which rests on it; there is a strip of iron fastened to the tube, on each side of this support, to prevent it from altering its position relatively to the lamp. The part of the tube *b* opposite to the wire gauze is cut out, so as to leave a clear space of half an inch all round for the passage of the explosive mixture. The pipe *h* conveys gas to the chamber *f*, and the pipe *k* supplies the jet in the inside of the wire gauze; the quantity is regulated by screw-clips on the india-rubber tubes.

The experiment is made in the following way:—A pistol, of which the barrel is $\frac{1}{2}$ in. bore and 5 in. long, is loaded with ·205 gramme of gunpowder, and several pieces of paper are rammed down well upon the charge; the firing is done by a cap. The gas-jet of the lamp having been lighted and the wire gauze screwed into its place, gas is made to pass into the chamber *f*, and escaping by the holes in the top, it mixes with the air and forms an explosive mixture, which surrounds the lamp: part of the explosive mixture passes into the interior, where it is ignited; the remainder passes up on the outside. The muzzle of the pistol is then placed in the hole in the wooden disk, and as soon as the shot is fired along the axis of the tube, a large flame leaps up, and continues to burn

Fig. 1.

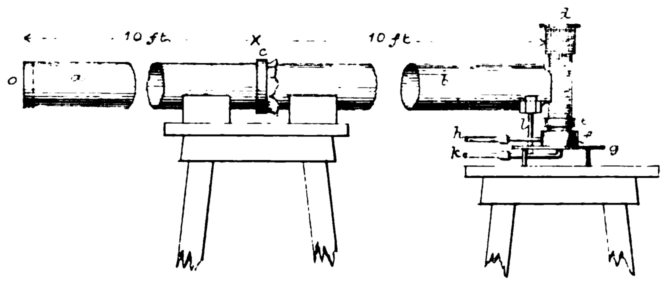


Fig. 2.

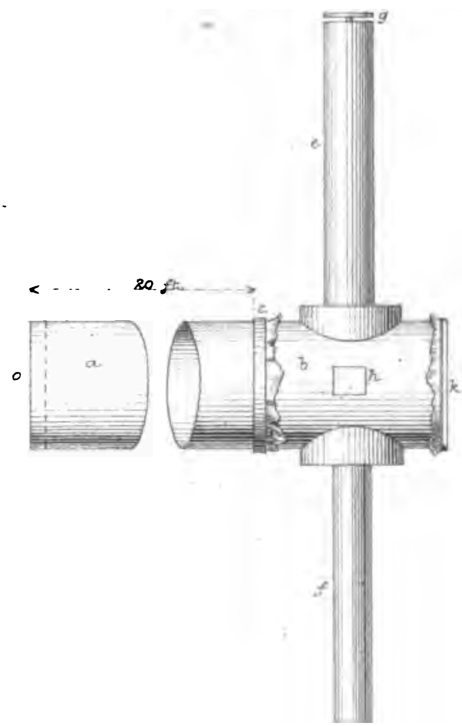
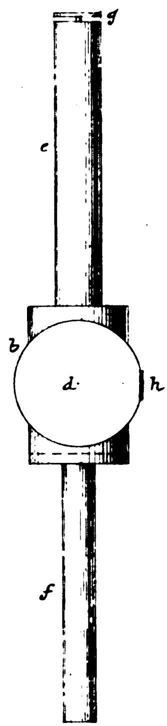


Fig. 3.



Scale 1 in. = 1 foot.

W. H. Wesley lith.

Fig. 4.

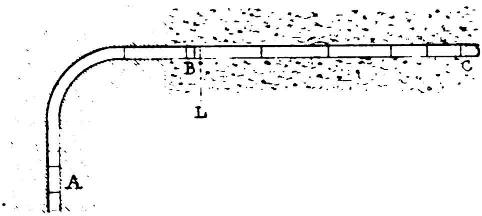


Fig. 5.

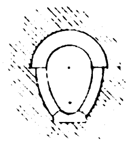
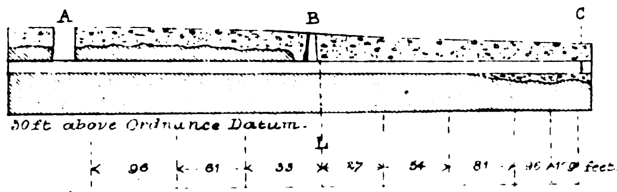
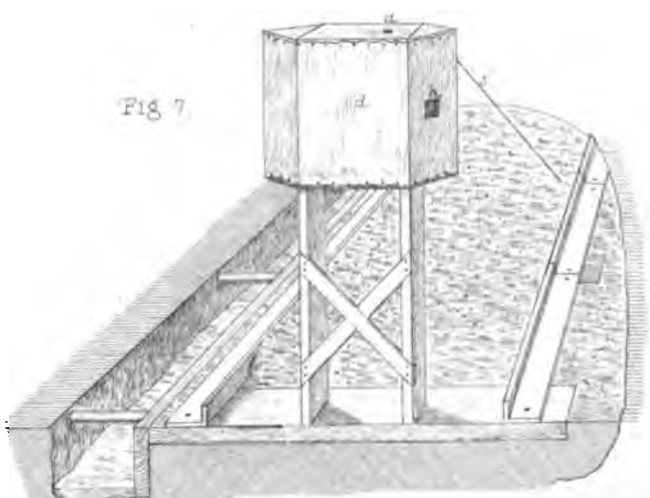


Fig. 6.



Scales.
Figs. 4 & 6, 1 in = 80 ft. Fig 5 1 in = 16 ft.



Scale ⅛ inch = 1 foot.

on the outside of the lamp*. If the charge of gunpowder be increased to ·272 gramme, or be decreased to ·136 gramme, the experiment does not succeed; and if the wire gauze has become smoked by the flame of the inner jet being too large, the flame cannot be passed through.

In the apparatus represented in fig. 2, there are again two tin-plate tubes, each 10 ft. long by 8 in. diameter, but they are joined to form one continuous tube 20 feet long. At the end *o* there is a disk of wood, $\frac{3}{4}$ in. thick, with a hole in the centre for the muzzle of the pistol. The tube *b* (figs. 2 & 3), of tin plate, 12 in. long, has its interior isolated by an india-rubber sheet tied over the end *c*, and a sheet of paper tied over the end *k*. A ring, with a network of wires $\frac{1}{8}$ in. thick, and with meshes $\frac{1}{4}$ in. square, is drawn over the diaphragm in the same way as in the apparatus already described. Two short tubes, of 6 in. diameter, are joined to *b* to form a chamber large enough to receive a safety-lamp; they are closed by flat ends, with the exception of a hole 3 in. diameter in the upper one, opening into a chimney *e*, and an opening of 2 in. diameter into the tube *f* in the lower one. The upper end of the tube *f* opens into a flat round chamber, with holes $\frac{1}{2}$ in. diameter and $\frac{1}{2}$ in. apart round about its outside; its position is indicated by the dotted line in fig. 3. At the top of the chimney *e* there is a draught regulator, *g*, which can be raised or lowered by means of the screwed spindle which supports it. The safety-lamp to be tested is placed on the discoid chamber, with its top projecting into the chimney if it is so long. Gas is supplied by a Bunsen burner at the bottom of the tube *f*, and, mixing with air, it flows upwards through the discoid chamber into the isolated space around the lamp. The products of combustion pass upwards through the chimney.

The experiment is made thus:—The pistol is loaded with ·41 gramme† of gunpowder in the same way as before: an ordinary Davy or Clanny lamp is lighted and put into the space *d*, which is afterwards closed at the ends. Gas is then made to flow into the tube *f*; the lamp is observed through the window *h*, and as soon as it is seen that the atmosphere in the space *d* is explosive, the shot is fired at *o*. The paper at *k* is blown out and set on fire; and the flame of the explosive mixture, passing backwards down the tube *f*, ignites the gas escaping from the Bunsen burner.

The lamps which were tested with this apparatus are those known as the Davy, Clanny, Stephenson, Mueseler, and Eloin. The flame was easily passed through the Davy lamp, with rather more difficulty through the Clanny, and not at all through any of the others.

The first experiments with firedamp were made in No. 7 Pit, Barleith, near Glasgow. A wooden plug, with a small pipe through it, was driven

* This experiment was shown by Mr. Spottiswoode at the Royal Institution on the evening of the 17th of January, 1873, with the apparatus I have described. The same apparatus was afterwards used at one of the Cantor Lectures of the Society of Arts.

† If the charge be made greater or less than this by ·15 gramme the experiment does not usually succeed.

into a horizontal borehole which had struck a blower, and the firedamp was conducted in tubes to a collecting vessel at a short distance. I soon found that this firedamp was very impure, as a mixture of one part of it with thirteen parts of air was not explosive; however, I made a number of experiments in the mine with both sets of apparatus (figs. 1 & 2, Plate VI.), but did not succeed in passing the flame, except perhaps in one doubtful instance with the larger apparatus, when the gas issuing from the Bunsen burner was not set on fire.

The next experiments with firedamp were made in the C Pit, Hebburn Colliery, near Newcastle-on-Tyne. The gas, which issued from a borehole similar to that in the Barleith Pit, was collected in the same way, and conveyed in the collecting vessel to a convenient place near the stables, where naked lights could be used. The experiments with both sets of apparatus were quite successful, the quantity of gunpowder required being, in each case, the same as when coal-gas was used. The Davy lamp employed in the experiments with the larger apparatus belonged to the colliery, and was in constant use below ground. At the fifth trial (when I had ascertained the quantity of gunpowder required) the flame passed through the wire gauze, set fire to the paper tied over the end *k*, and passing backwards down the tube *f*, kindled the gas issuing from the Bunsen burner. My brother, Mr. R. L. Galloway, who was the resident viewer of the colliery at that time, was observing the lamp through the window *h* when the shot by which the flame was passed was fired. The flame of the wick, which was of ordinary dimensions before it was surrounded by the explosive mixture, had sent up a long smoky point to near the top of the gauze, which showed that the explosive mixture was composed of about 1 part of firedamp to 12 or 13 parts of air. The lamp was carefully examined after the trial, and was found to be in good order.

The Directors of the Company to whom the colliery belongs were unwilling to allow any further experiments to be made in the mine, so that this series had to be abandoned before any more results had been obtained.

Following are the analyses of the firedamp used in the foregoing experiments. The sample of gas from the Barleith blower was collected by myself at the time the experiments were being made, and analyzed by Dr. T. E. Thorpe, of Glasgow; that from the Hebburn blower was collected by my brother several weeks before the experiments, and was analyzed by Dr. Wright, of St. Mary's Hospital, London.

	Barleith.	Hebburn.	
Light carburetted hydrogen	75·86	85·22	
Carbonic acid	1·31	3·27	
Olefiant gas	..	traces	
Carbonic oxide	..	1·36	
Oxygen	..	2·17	} 1·51
Nitrogen......................	22·83	7·98	
	100·00	100·00	

The next experiments were on a larger scale. Through the kindness of Mr. Carrick, the City Architect of Glasgow, part of a new sewer in North Woodside Road was placed at my disposal; and Mr. Foulis, the manager of the Corporation Gas-Works, caused a pipe to be led into it, and provided a liberal supply of gas. Figs. 4, 5, & 6, Plate VII., are sections of the part of the sewer in which the experiments were made: fig. 4 is a plan section through the widest part, fig. 5 is a vertical cross section showing the dimensions of the sewer (6 ft. × 4 ft. are the greatest measurements), and fig. 6 is a vertical longitudinal section through the highest part. Part of the sewer is a tunnel in solid rock (the diagonal shading in fig. 6 shows the position of the rock), and part of it is built in brickwork through the surface-drift. The length that was available for the experiments is comprised between the point A, where there was a wide shaft to the surface, and the point C, where I caused a wooden partition to be set up to prevent the draught of air from affecting the lamp. B is a manhole, 3 ft. 6 in. × 3 ft. 9 in. at the bottom, and 23 in. square at the top; it was covered by two stones, each about 2 in. thick, with a space about 1 in. wide between them across the middle of the top of the manhole. The safety-lamp part of the apparatus (fig. 1, Plate VI.) was set upon a board fixed across the sewer at the point L, at a height of 2 ft. 8 in. from the deepest point.

I made a large number of experiments here, but it will be sufficient to give only the principal results. The shots were fired from the same pistol that was employed in the former experiments at the distances from the lamp indicated by the figures below fig. 6, Plate VII.; they were nearly all fired towards the position of the manhole B. Each measure of gunpowder weighed ·273 gramme (=4·213 grains). The number of measures given below, corresponding to the distances from the lamp at which the shots were fired, are those by which the flame was passed; and it is to be understood that at each distance a charge containing one measure less was generally insufficient to effect the purpose.

(1) Between C and L:—

At	27 ft.	5 measures	= 1·365 gramme	
	54 ft.	8 ,,	= 2·184 grammes	
	81 ft.	10 ,,	= 2·730	,,
	96 ft.	12 ,,	= 3·276	,,
	109 ft.	14 ,,	= 3·822	,,

One experiment was made with the pistol pointing towards the roof at an angle of 70° to the axis of the sewer; the distance was 109 ft., the charge 20 measures, = 5·460 grammes; the muzzle of the pistol was 1 ft. 6 in. from the floor, and the firing was effected by drawing a cord. The flame passed through the wire gauze, and ignited the gas on the outside.

(2) Between A and L:—

At 33 ft.	8 measures	=	2·184 grammes
61 ft.	8 ,,	=	2·184 ,,
96 ft.	8 ,,	=	2·184 ,,

It is remarkable that, in these latter experiments, it was not necessary to increase the quantity of gunpowder as the distance from the lamp was increased. The large charge required at the first station seems to have been owing to the presence of the manhole between the lamp and the point at which the shot was fired; but this waste of energy having been provided for, no further addition to the charge was required. It would seem as if part of the energy of the sound-wave was expended in the space C L in shaking the brickwork and a narrow wooden gangway supported on cross-pieces at a height of 1 ft. 5 in. from the sole; whereas in the space A L, in which no gangway had been laid down, it was conveyed through the tunnel in the solid rock without much loss of intensity.

The temperature of the air in the sewer was 55°–56° Fahrenheit; and there was generally a current travelling in the direction C to A at the rate of 5 to 10 ft. per minute.

These are the last experiments from which important results have been obtained; they were concluded in November 1873.

After this I made some experiments with firedamp in a stone-mine in No. 2 Pit, Douglas, near Glasgow. I filled a sheet-iron box of 18 cub. ft. capacity with firedamp at the borehole in the C Pit of Hebburn Colliery, and brought it to this mine. As the gas appeared to have become mixed with air through leakage during the transport, and would not burn satisfactorily in the lamp of the apparatus (fig. 1, Plate VI.), the apparatus shown in fig. 7, Plate VII., was constructed. Two boards, each $\frac{5}{8}$ of an inch thick, and of the shape and dimensions of the top of the apparatus, are joined together by iron rods $\frac{3}{8}$ of an inch in diameter, one at each angle. A sheet of india-rubber, $\frac{1}{32}$ of an inch thick, is then fastened round the frame thus formed by nailing it to the boards, and an isolated space of the form *d*, fig. 7, is obtained. An opening, $1\frac{5}{8}$ inch in diameter, in the upper board serves as an outlet for the products of combustion; and a similar opening in the lower board serves as an inlet for fresh air and the firedamp from a Bunsen burner. This apparatus is placed on two legs fastened to one of the sleepers in the roadway, and it is stayed tightly before and behind by four stout wires in positions analogous to *s*, the only one that can be seen in the figure.

A Davy lamp was lighted and placed in the inside of *d*, on a block of wood 3 inches high by 3 inches in diameter, so as to have its wire gauze as near as possible to the centre of the space; firedamp was then admitted at the lower opening, and the draught was regulated at *a*. The appearances presented by the lamp were observed through a glass window, *h*, fastened in the sheet of india-rubber; and as soon as the flame showed

that the mixture surrounding it was explosive, shots were fired from a gun at a distance of 30 yards. The barrel of the gun which was used is $\frac{13}{16}$ of an inch in diameter, and it is rifled for a length of 3 ft. with seven grooves; the breech which received the charge is smooth-bored, and $4\frac{1}{8}$ inches long. Each measure of gunpowder weighed 3·822 grammes (=59 grains), and the charges fired ranged between 1 and 9 measures; paper tamping was rammed down tightly, and the charge was fired by a cap.

The gun was tied to a prop in the middle of the mine, with its barrel at an angle of about 35° upwards, pointing towards the apparatus; the muzzle was 18 inches from the floor. At the part where the experiments were made, the sizes of the mine are:—width at top, 4 ft.; width at bottom, 6 ft.; height, 5 ft. 6 in.

The sound-wave from a shot of two measures extinguished the flame of the Davy lamp when it was placed on the outside of the apparatus; but when it was placed in the inside of *d*, the flame could not be extinguished nor passed through the meshes, even when the quantity of gunpowder was raised to nine measures. However, after the lamp had been allowed to burn in the isolated space for a few minutes (the supply of fresh air not being very good), its flame could be extinguished by the sound-wave from a shot of four measures. The whole quantity of fire-damp was so small that there was no opportunity for enlarging or varying the apparatus.

These experiments, and one which I made formerly in the sewer with the *b* tube of the apparatus, fig. 2, Plate VI., show that a very slight obstacle will interfere with the action of the sound-wave. They were concluded in March 1874.

I would add, in concluding, that the liberal grant of money which I received from the Government-Grant Committee of this Society has been of great value in enabling me to carry out these experiments.

I have also been much indebted for assistance to each of the following gentlemen:—Mr. Robert H. Scott, F.R.S.; Professor A. C. Ramsay, F.R.S.; Professor W. W. Smyth, F.R.S.; Professor Marreco, of the College of Physical Science, Newcastle-on-Tyne; Mr. John Galloway, of Barleith and Dollars Collieries; Mr. J. B. Simpson, of Newcastle-on-Tyne; Mr. Charles Shute, of Hebburn Colliery; and to Mr. William Kirkwood, of the Inkerman Mines, near Glasgow.

XX. "On the Adiabatics and Isothermals of Water." By A. W. Rücker, M.A., Fellow of Brasenose College, Oxford. Communicated by R. B. Clifton, M.A., F.R.S., Professor of Experimental Philosophy in the University of Oxford. Received June 4, 1874.

M. Verdet, in his work on Thermodynamics ('Œuvres,' vol. vii. p. 184), enunciates the proposition "Deux courbes de nulle transmission ne peu-

vent se couper," and offers a proof which rests upon the assumption that if a body could undergo a series of operations represented as to the changes of pressure and volume by PQMP (where PQ is an isothermal and PM, QM two adiabatics), no heat would be gained or lost at any part of the cycle except PQ.

It is, however, evidently impossible that the body could, at the point M, pass from one adiabatic to another without absorbing or emitting heat, *i.e.* while fulfilling the very condition that it should *not* pass from one adiabatic to another; and the question as to the possibility of the intersection of two adiabatics must therefore be submitted to a more general investigation, as it is certainly conceivable that heat might be gained or lost during the passage from the point M considered as lying on the first curve to the point M considered as belonging to the second, whether it took place, as supposed by M. Verdet, without any accompanying changes of pressure or volume, or whether, as we shall see would be generally the case, it could only be accomplished if the body were caused to assume a series of intermediate states involving such changes.

The question admits of an easy answer if we consider the case of bodies which can exist in two distinct states under the same circumstances of pressure and volume; and for the present we may confine our attention to water, which is the most conspicuous representative of the class, and which, at the ordinary atmospheric pressure and at temperatures between 0° C. and 4° C., exists in a series of states in which the volumes are the same as those which it assumes if heated at the same pressure from 4° C. to about 8° C.

Hence whereas for higher temperatures all the properties of water at atmospheric pressure are completely defined if we know the volume, such is not the case between the limits above indicated; but each point on the line of constant pressure given by $p=1$ atmosphere between its intersections with the isothermals 0° C. and 4° C. corresponds to two states of the water, or rather, since if the water-substance be converted into ice it will, if cooled sufficiently, again pass through the same range of volumes, each point corresponds to three states and is the intersection of three isothermals; and as a similar remark may be made with respect to neighbouring lines of constant pressure, it follows that there is a region in the plane of pv such that three states of the water-substance correspond to each point within it, and that therefore the values of p and v given by any such point do not define the state of the water.

If, however, from every point in the plane of pv we draw perpendiculars to that plane, proportional to those values of some other property of the water (say, in this case, its temperature) which correspond to the conditions of pressure and volume represented by the points from which they are drawn, the extremities of such ordinates will form a surface which will be met once, or more than once, by any particular ordinate, according as the water can exist under the circumstances of pressure

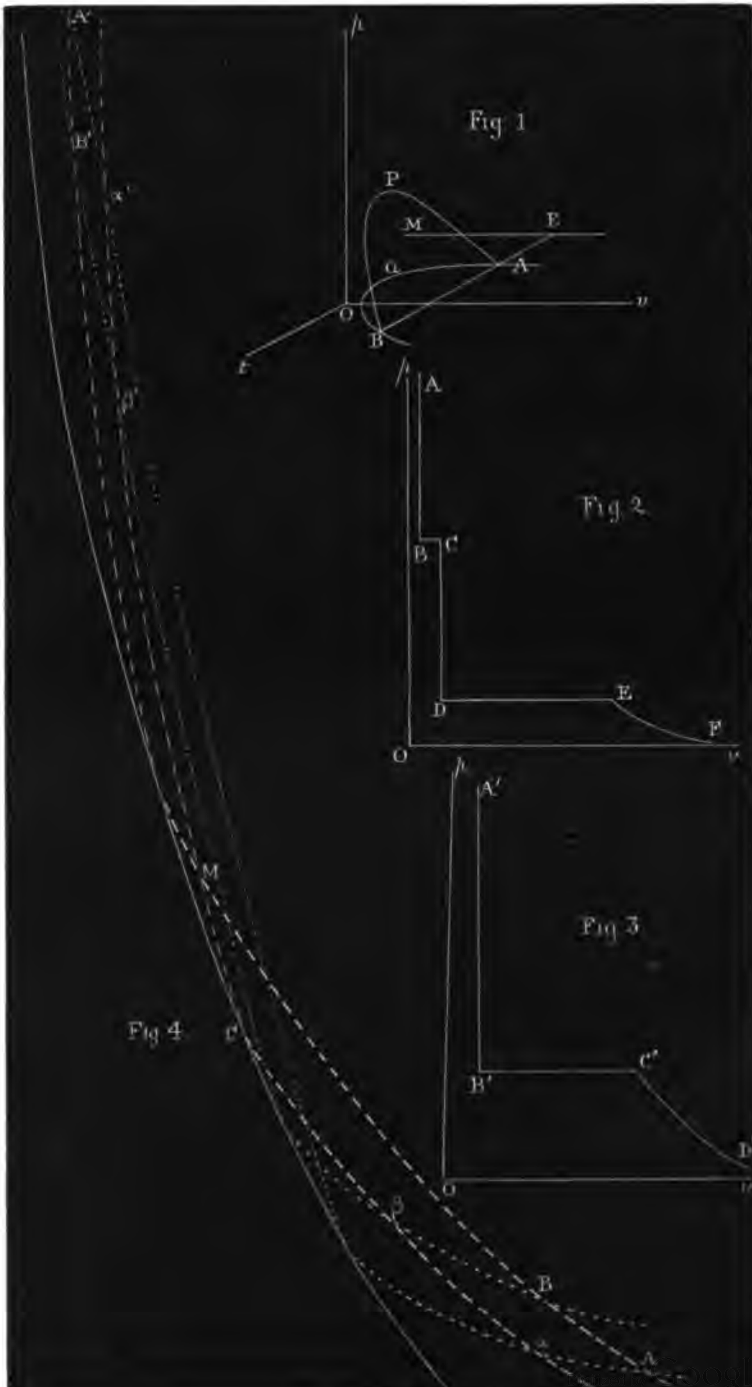


Fig 1
p
P
M
E
Q
A
O
v
B
t
Fig 2
p
A
B
C
D
E
F
O
v
Fig 3
p
A′
B′
C′
D′
O
v
Fig 4
A′
B′
M
C
B
A

and volume defined by the point in the plane of pv from which it is drawn in only one or in several states.

This surface will be represented by the equation

$$f(pvt)=0\ ;$$

and curves may be drawn on it showing the relations between the pressure, volume, and temperature when the state of the water is altered in any determinate manner, the projections on the plane of pv of those represented by the equation to the surface, combined with

$$\left(\frac{df}{dt}\right)=0,$$

forming the boundaries of the region from all points in which ordinates can be drawn parallel to the axis of t which intersect the surface in two or more points. The ordinary adiabatics drawn on the plane of pv are the orthogonal projections of curves on the surface, each of which is defined by the condition that the water in passing through the series of states indicated by its successive points neither gains nor loses heat, and which, to avoid confusion, will be called *complete* adiabatics.

Let now the line LM in the plane of pv (fig. 1) be the line $p=1$ atmosphere. Draw an ordinate from L meeting the surface in A and B; then, if different complete adiabatics pass through A and B, their projections on the plane of pv will intersect; and the only hypothesis on which we can avoid the assumption of the intersection of adiabatics is that the complete adiabatics are the intersections of the characteristic surface $f(pvt)=0$ with cylindrical surfaces, the director curves of which are the plane adiabatics, and the generating lines parallel to the axis of t. In this case the same complete adiabatic would pass through every such pair of points as A and B, which is evidently impossible, as in performing the cycle AQBPA the water would absorb heat along AQB without at any time emitting it, and yet would neither increase its internal energy nor perform any external work, since the cycle projects into a straight line and a discontinuous curve meeting it in only one point. As, therefore, a complete adiabatic cannot pass through A and B, and as a similar train of reasoning would hold for the third point in which AB meets the surface, three adiabatics as well as three isothermals pass through the point on the plane of pv, which is the common projection of these points.

As this conclusion disproves M. Verdet's theorem, we may proceed to consider a few simple propositions based on the hypothesis of the possibility of the intersection of adiabatics; and in so doing it will be advisable to use a new term to distinguish between two classes of points of intersection of the projections on the plane of pv of curves on the surface; and reserving the usual expressions (intersect, cut, meet, &c.) for the projections of points of intersection on the surface, we shall say that two curves *cross* one another when they meet in a point which does not correspond

to any such point of intersection, but is only the common projection of two separate points on the surface.

In the first place, then, we know that if water, starting from an initial state such that addition of heat at constant pressure is accompanied by diminution of volume, be allowed to expand without receiving or emitting heat, its temperature will rise; *i. e.* it will at the same time be doing work, solely at the expense of its internal energy, and rising in temperature—a process which cannot go on indefinitely, as at last all the internal energy would be due to the temperature alone, and any further performance of work would necessarily involve a fall in temperature.

Hence there must be a point of maximum temperature on the complete adiabatic drawn through the point representing the initial state; and the isothermals through all other points on the same curve which lie within the region, in which addition of heat involves contraction, must meet it twice. The projections of these curves will also necessarily intersect in two points; and since when an adiabatic and isothermal meet the tangent to the former always makes the larger acute angle with the axis of v (Maxwell, 'Theory of Heat,' p. 130), it follows that the two curves must also cross at some point between their points of intersection, and will thus form two loops.

This result holds however near the points of intersection may be together; and when they coincide the curves on the characteristic surface touch one another, and their projections on the plane of pv have contact of the second order, since three points, *i. e.* the two points of intersection and the crossing point, are coincident; and, further, the isothermal which thus touches the adiabatic is evidently that which corresponds to the maximum temperature above mentioned; and the point of contact lies on the curve which is the boundary between the regions in which elevation and depression of temperature are respectively the results of compression, for at neighbouring points on the adiabatic the temperature is lowered when the volume is either increased or diminished.

All the points of maximum temperature on the complete adiabatics lie on the curve defined by the condition

$$\left(\frac{df}{dt}\right)=0\,;$$

and since at all points on this curve the tangent planes to the surface are perpendicular to the plane of pv, therefore the projections on that plane of all curves intersecting it touch its projection, because their tangents lie in a plane perpendicular to that of pv, and are projected into one line.

Hence the projection of any curve which meets this curve must at the projection of the point of section touch an adiabatic.

But the ordinary interpretation put upon contact of an odd order with an adiabatic is that the body passing through the cycle of operations

represented by the curve, at the point of contact ceases to emit and begins to absorb heat, or *vice versâ*; and that therefore every closed cycle, if a continuous curve, must have $2n$ points of contact of an odd order with adiabatics, and if a discontinuous curve, $2n-r$ such points of contact and r points of discontinuity at which the curve does not cut the adiabatics passing through them.

This, however, evidently does not hold for a curve which meets the curve in the plane of pv, defined by

$$\left(\frac{dv}{dt}\right)=0,$$

which is the projection of the curve in space, whose equations are

$$f(pvt)=0, \left(\frac{df}{dt}\right)=0.$$

For since it does not follow that the curves in space have contact because their projections touch, we see that the curve in the plane of pv may touch an adiabatic without any change taking place in the absorption or emission of heat; and such a curve may, even if continuous, have contact of an odd order with an odd number of adiabatics. The point of contact, for instance, of a curve which touches but does not intersect the limiting curve at all points on which $\left(\frac{df}{dt}\right)=0$, projects into a point of contact of the third order at least; and therefore the projected curve must lie entirely between the adiabatic and projection of the limiting curve, which only have contact of the first order—*i. e.* it has a single point of contact of an odd order, with an adiabatic which does not correspond to a change in the absorption or emission of heat, and therefore on the whole it has an odd number of points of contact of an odd order with adiabatics.

Let us now suppose that $ABB'A'$ and $\alpha\beta\beta'\alpha'$ are two adiabatics (fig. 4) which meet the curve $\left(\frac{dv}{dt}\right)=0$, and let two isothermals, $A\alpha\alpha'A'$ and $B\beta\beta'B'$, meet the first in AA' and BB' and the second in $\alpha\alpha'$ and $\beta\beta'$ respectively. We can now make the water go through Carnot's cycle of operations between the same temperatures in *four* different ways, of which we need only consider the cycles $\alpha'A'B'\beta'$ and $\alpha'A'B\beta$. In each of these the quantities of heat received along $\alpha'A'$ are the same, therefore the quantities of work done must be the same, *i. e.*

$$\begin{aligned}\text{area } \alpha'A'B'\beta'\alpha' &= \text{area } \alpha'A'B\beta\alpha' \\ &= \text{area } \alpha'A'B'\beta'\alpha' + \text{area } \beta'B'B\beta\beta'; \\ \therefore\ &\text{area } \beta'B'B\beta\beta' = 0.\end{aligned}$$

But this area is composed of the two $B'\beta'M$ and $B\beta M$, and they are of opposite signs; for in going round the closed curve $M\beta'B'M$, the work done on the body is greater than the work done by it, while in the loop $MB\beta M$ the contrary is the case; whence we conclude that the areas $B'\beta'M$ and $B\beta M$ are equal.

In the figure the point β' is represented as further from C than M is; if, however, β' lies between M and C, then the crossing point of the isothermal and adiabatic must be substituted for that of the two adiabatics; and in any case the areas of the two loops formed by two adiabatics and an isothermal which meets each of them twice are equal.

This result will still hold if we suppose that the two points of intersection with one of the adiabatics coincide, *i. e.* that it is that one which has contact of the second order with the isothermal; whence it follows that the areas of the loops formed by any adiabatic and an isothermal which meets it twice are equal; or, in other words, that

If a body perform a cycle of operations which can be represented by an adiabatic and an isothermal, it will on the whole do no useful work.

If we now proceed to consider the shapes of the adiabatics and isothermals of water near their points of section with the curve which is the second boundary between the regions in which addition of heat causes respectively increase and diminution of volume, and which corresponds for any given pressure to a local maximum as that already discussed does to a minimum volume, the applications of several of the above remarks are too obvious to need any special comment; but there is one isothermal the relations of which to the adiabatics which intersect it are of a very complex order, and to which therefore it may be well to draw attention. The isothermals of water may be divided into two classes, according as the pressure corresponding to the freezing-point is or is not less than the maximum tension of aqueous vapour at the given temperature.

As a type of the first we may take the isothermal corresponding to 0° C., which is represented in fig. 2. The maximum tension of steam at this temperature is 4·6 millims.; and as this is less than the pressure at the freezing-point, the vapour will be directly precipitated into ice, which will in turn be converted into water, when the pressure amounts to 760 millims., the solid being thus intermediate between the gaseous and liquid states.

An isothermal of the second class is represented in fig. 3. In this case the vapour is precipitated in the form of water; and as the possibility of the existence of water at the given temperature and pressure proves that the freezing-point for the given pressure is below the temperature proper to the isothermal, and as any further increase of pressure will tend still further to depress it, it is evident that the water-substance can never exist in the solid state at the given temperature unless at very great pressures contraction instead of expansion accompanies solidification. There must, therefore, be some isothermal which is at once the boundary and limiting form of these two classes; and if considered as belonging to the first, it will be that for which the portion CD disappears, *i. e.* for which the pressures corresponding to the freezing- and boiling-points are the same.

The form of this curve will therefore be that of an isothermal of the second class; but for the pressure corresponding to B′C′ the water-substance can exist in all three states; and as the portion of the curve in space corresponding to B′C′ is a line perpendicular to the plane of pt, its projection on that plane is the triple point of Professor James Thomson; and if we assume, with him, that ice, water, and steam can all exist together at the temperature and pressure in question, it follows that this line is both an isothermal and adiabatic; for if we suppose the water-substance to exist at the same time in all three states in a vessel impermeable to heat, we can evidently by diminishing the volume convert some of the steam into water, and employ the heat so set free in melting a portion of the ice, during which operation the state of the mixture will always correspond to a point on B′C′.

Not only, however, is a single adiabatic coincident with the isothermal, but all the adiabatics within certain limits pass through each point on B′C′, and are for a certain distance coincident with it, and therefore with each other; for as the conversion of ice into water is accompanied by contraction, and that of water into steam by expansion, we can keep the volume and pressure of a mixture of ice, water, and steam constant, while, by supplying or subtracting heat, we alter their relative proportions.

The mixture can thus be made to go through Carnot's cycle without any change either in the pressure or temperature, the result always being that no useful work is done; and as in the earlier portion of this paper it has been shown that it is possible for two adiabatics, drawn as plane curves, to intersect, so now we have an instance of the intersection of complete adiabatics, all three variables p, v, and t, to which points on these curves are referred, being insufficient to determine the state of the water-substance along the line B′C′.

It is easy to determine the points at which the adiabatic corresponding to any given mixture enters and leaves B′C′.

Let σ, s, and Σ be the specific volumes of the ice, water, and steam, r and ρ the latent heats of conversion of ice into water and steam respectively, and v the volume of a kilogramme of the water-substance, when the proportions by weight of steam, water, and ice are

$$\xi : x : 1-x-\xi.$$

We have then, as the temperature is constant,

$$dQ = rdx + \rho d\xi,$$

and

$$v = \Sigma\xi + sx + \sigma(1-x-\xi).$$

If no heat is supplied or abstracted,

$$dQ = 0 \text{ and } r(x-x_0) + \rho(\xi-\xi_0) = 0.$$

If we consider x_0 and ξ_0 to belong to the initial state, two cases arise according as

$$\xi_0 \text{ is or is not } > (1-x_0)\frac{r}{\rho},$$

i. e. according as there is or is not enough steam to supply by its condensation a sufficient quantity of heat to melt all the ice; and as

$$\frac{dv}{d\xi}=\Sigma-\sigma-(s-\sigma)\frac{\rho}{r},$$

which is always positive, as $s-\sigma$ is negative, we have the largest and smallest volumes given by the limits

$$x=0 \text{ and } 1-x-\xi=0,$$

or

$$x=0 \text{ and } \xi=0.$$

The maximum volume is therefore in any case given by

$$(\Sigma-\sigma)\frac{rx_0+\rho\xi_0}{\rho}+\sigma,$$

and the minimum volume is

$$(\Sigma-s)\frac{r(1-x_0)-\rho\xi_0}{r-\rho}+s \text{ in the first}$$

and

$$(s-\sigma)\frac{rx_0+\rho\xi_0}{r}+\sigma \text{ in the second case;}$$

and the differences between these quantities give the range of volumes for which the adiabatic belonging to the initial values x_0, ξ_0 coincides with the isothermal.

In conclusion it is only necessary to point out that some of the results in the earlier part of the paper follow immediately from the ordinary formulæ of thermodynamics.

If C_p and C_v are the specific heats at constant pressure and constant volume respectively, and if, to avoid confusion, we write the quantity which is supposed to remain constant as a subscript to a partial differential coefficient, we have the well-known expressions

$$C_v=C_p-AT\left(\frac{dv}{dt}\right)_p\left(\frac{dp}{dt}\right)_v$$

and

$$\left(\frac{dv}{dp}\right)_Q=\frac{C_v}{C_p}\left(\frac{dv}{dp}\right)_t,$$

where Q is constant for any adiabatic. From the first it follows that when $\left(\frac{dv}{dt}\right)=0$,

$$C_v=C_p,$$

and

$$\therefore\left(\frac{dv}{dp}\right)_Q=\left(\frac{dv}{dp}\right)_t,$$

i. e. the adiabatics and isothermals touch one another at points of maximum or minimum volume.

Also by differentiation,

$$\left(\frac{d^2v}{dp^2}\right)_Q=\frac{C_v}{C_p}\left(\frac{d^2v}{dp^2}\right)_t+\frac{d}{dp}\left(\frac{C_v}{C_p}\right)\left(\frac{dv}{dp}\right)_t;$$

whence for all points on the curve $\frac{dv}{dt}=0$, we have

$$\left(\frac{d^2v}{dp^2}\right)_Q=\left(\frac{d^2v}{dp^2}\right)_t,$$

and therefore the contact is of the second order.

P.S. Since the above was written, a paper has been published in the 'Annales de Chimie et de Physique' for March 1874, in which the author, M. J. Moutier, is led, from thermodynamical considerations, to the conclusion that it is impossible for aqueous vapour in contact with ice to have the same tension as when it is in contact with water at the same temperature; and as some conclusions have been pointed out in the preceding pages which follow on the assumption that at the triple point the tension of the vapour is the same in each case, it may be well to show that his arguments do not really touch the question as to which of the two hypotheses is the true one.

M. Moutier discusses the case of a body which can exist in two different states, M and M′, such as the solid and liquid; and supposing that the tension of the vapour is different according as it is in contact with the first or second, he obtains a general formula for the heat of transformation from M to M′, from a consideration of the quantities of heat gained or lost if the body is compelled to undergo a definite series of changes constituting a closed cycle (p. 348).

The second operation in this cycle is that the body M′ passes from the pressure ϖ to the pressure p'; and in the application of the general formula to the case of water, M is taken to represent ice at 0° C., M′ liquid water at the same temperature, ϖ the atmospheric pressure, and p' the tension of aqueous vapour over liquid water at 0° C. (p. 362).

If, however, the symbols have these meanings, the prescribed operation is, in the case of water, impossible; for as water cannot exist at 0° C. in the liquid state at less than the atmospheric pressure, the body M′ would be converted into M as soon as the pressure ϖ was diminished, and no conclusions can be drawn from the cycle in question in the case of water.

M. Moutier employs a second argument which can be shown to have no greater weight than that already discussed, and which may be stated as follows:—

If Q is the latent heat of conversion of ice into water, and L and L′ the latent heats of conversion of ice and water respectively into steam, then at the triple point we must have

$$Q=L-L'.$$

L and L′ are given by the well-known formulæ

$$L=AT(v-u)\frac{dp}{dt},$$

$$L'=AT(v'-u')\frac{dp'}{dt},$$

where u and u' are the specific volumes of ice and water, and p, p', v, and v' the pressures and specific volumes of steam over ice and water respectively.

At the triple point $v=v'$ and $p=p'$; and M. Moutier further assumes that $\frac{dp}{dt}=\frac{dp'}{dt}$, and therefore obtains by substitution

$$Q=AT(u'-u)\frac{dp}{dt};$$

and as $\frac{dp}{dt}$ is positive, being derived from formulæ which have reference to the maximum tension of the vapour, and $u'-u$ is negative, it follows that Q, or the latent heat of water, is negative, a result which shows that some of the premises must be false.

The erroneous assumption, however, is not the possibility of the existence of the triple point, but is contained in the equation

$$\frac{dp}{dt}=\frac{dp'}{dt};$$

for Professor James Thomson has recently shown (Proc. Royal Society, Dec. 11, 1873) that M. Regnault's experiments, on the whole, favour the conclusion, which he draws from theoretical considerations, that

$$\frac{dp}{dt}=1\cdot13\frac{dp'}{dt};$$

and if this equation be true,

$$Q=AT\left\{(v-u)\,1\cdot13-(v-u')\right\}\frac{dp'}{dt}$$
$$=AT\left\{0\cdot13v-1\cdot13u+u'\right\}\frac{dp'}{dt};$$

whence, as at 0° C., $v=210\cdot66$, while u and u' differ little from 0·001, it is evident that for a temperature so near zero as that of the triple point, the expression within the brackets must be positive, and Q is, as it should be, positive also.

XXI. "Contributions to Terrestrial Magnetism."—No. XIV. By General Sir EDWARD SABINE, R.A., K.C.B., F.R.S. Received June 18, 1874.

(Abstract.)

This paper is presented by the author as No. XIV. of his "Contributions to Terrestrial Magnetism," completing the magnetic survey of the northern hemisphere (of which No. XIII. comprised the higher latitudes). It consists of a very brief explanatory introduction, followed by Tables, in which (as in No. XIII.) the three magnetic elements are arranged in zones of latitude. These Tables, which form the body of the work, are accompanied by three maps, presenting the results *graphically*, in isogonic, isoclinal, and isodynamic lines.

XXII. "Tables of Temperatures of the Sea at various Depths below the Surface, taken between 1749 and 1868; collated and reduced, with Notes and Sections." By JOSEPH PRESTWICH, F.R.S., F.G.S. Received June 4, 1874.

(Abstract.)

This paper was commenced by the author more than twenty years since, with a view to the geological bearing of the subject, but was for some years unavoidably interrupted. It has now been brought down to 1868, the date of the 'Lightning' expedition, when the subject was taken up by Dr. Carpenter, by whom it has since been so ardently and ably carried on. Nevertheless, as Dr. Carpenter's work relates almost solely to recent investigations, the author considers that there is yet considerable interest attached to the work of the earlier observers from 1750 to 1868, though he feels that much of it is necessarily superseded by the great and more exact work subsequent to 1868. He is aware that the older observations have also not been deemed reliable on account of the error caused by pressure on the thermometers at depths; but this is far from applying to the whole of them, as that error was taken into account so early as 1836, if not before, and a large number of these observations are equally reliable with the more recent ones, while the greater part of the others admit of corrections which render them sufficiently available.

In 1830, Gehler gave a list of 226 observations, and D'Urville, in 1833, tabulated 421 experiments according to depths. The present paper contains a record of about 1300 observations, which are arranged according to the degrees of latitude:—1st, for the northern hemisphere; 2nd, the southern hemisphere; 3rd, inland seas. They are all reduced to common scales of thermometer, measure of depth, and meridian. Their position is given on a map of the world, and the bathymetrical isotherms from the Poles to the Equator, based on the correct and corrected observations, are given in a series of ten sections. The author does not claim for these observations the exact value, or the unity and completeness of plan, of the more recent ones, while, as compared with them, the depths at which they were made are on the whole very limited; still they include a few at great depths; and as they extend over much ground that has not been covered by the expeditions of the 'Lightning,' 'Porcupine,' and 'Challenger,' he trusts that these Tables may be of some use as complemental to these later researches, and as bringing together and reducing to a common standard, observations scattered through a large number of works and memoirs. At the same time, the author would observe that he thinks it due to our many distinguished foreign colleagues who have been engaged in the inquiry, and whose work seems but little known, that the results of their researches should be understood in this country. Their conclusions, which are in close agreement with those formed, entirely in-

dependently, upon recent and better data by Dr. Carpenter, acquire, from this concordance, additional force and value. The author was not at all aware himself, in the earlier part of the inquiry, how much had been done, and often found himself framing hypotheses which, on further examination, he found had been long before anticipated by others.

The first part of the paper consists of an "Historical Narrative," which embraces an account of the character, number, and position of the observations made by Ellis (1749), Cook and Forster (1772), Phipps (1773), Saussure (1780), Péron (1800), Krusenstern (1803), Scoresby (1810 and 1822), Kotzebue (1815), Wauchope (1816 and 1836), John Ross and Sabine (1817 and 1822), Abel (1818), Franklin and Buchan (1818), Parry (1819, 1821, 1827), Sabine (1822), Kotzebue and Lenz (1823), Beechey (1825), D'Urville (1826), FitzRoy (1826), Blossville (1827), Graah (1828), Bérard (1830), Vaillant (1836), Du Petit Thouars (1836), Martins and Bravais (1838), Wilkes (1839), James Ross (1839), Belcher (1843 and 1848), Aimé (1844), Kellett (1845), Spratt (1845–1861), Dayman (1846), Armstrong (1850), Maury, Rogers, Bache (1854–57), Pullen (1857), Wüllerstorf (1857), Kündson (1859), E. Lenz (1861), Shortland (1868), Chimmo (1868).

The second part relates to the "Method and Value of the Observations." Wanting a reliable self-registering thermometer, the early observers, for a considerable time, used a machine contrived by Dr. Hales to bring up water, by means of a bucket with valves, from the depth at which the temperature was to be taken. This was used by Ellis, Cook, Scoresby, Wauchope, and Franklin, and one of a form improved by Parrot was employed by Lenz. Scoresby's observations in the seas around Spitzbergen are of much interest. He showed that while at the surface the temperature varied from about 29° to 42°, the temperature at depths of from 2000 to 4000 feet was generally about 34° to 36°; and there is reason to believe that, with the very slight corrections suggested by Lenz's subsequent researches, most of them are correct within a fraction of a degree.

The most remarkable readings, however, taken with this apparatus were those obtained by Lenz in Kotzebue's expedition of 1823. He applied to the observations a correction founded on Biot's law of the variations of temperature experienced by bodies in passing through mediums of different temperature, and determined the lowest temperatures hitherto noted in intertropical seas. Thus, one sounding in mid-Atlantic, 7° 21′ N. lat., at a depth of 3435 feet, gave a corrected reading of 35°·8 F., and another at a depth of 5835 feet, in mid-Pacific, 21° 14′ N. lat., gave 36°·4 F., the surface temperatures being 78°·5 and 79°·5. His observations on the specific gravity of sea-water are also valuable.

Saussure and Péron used thermometers surrounded with non-conducting substances, so that they might pass through the warmer upper strata of water with little change. Saussure's experiments deserve notice, inasmuch as, after applying a correction, they recorded, at that early period, for

the Mediterranean, at a depth of 1000 to 2000 feet, a temperature, so nearly right, of 55°·5.

Sir John Ross and Admiral Spratt sometimes used Six's thermometers, and at others took the temperature of the silt brought up from the bottom. The former obtained readings of 28°·5 F. for Baffin's Bay, and the latter of about 55° for the Grecian archipelago, agreeing therefore closely with good thermometrical observations.

Phipps used a differential overflow thermometer invented by Cavendish, but it was not found to answer. This form of instrument remained in abeyance until a greatly improved form of it was contrived by Walferdin (*thermomètre à déversement*) in 1836. It was used by Martins and Bravais in the Arctic seas, and by Aimé in the Mediterranean, and was said to give very satisfactory results. Aimé also used another somewhat similar instrument, which, at a given depth, was reversed and then hauled up. These instruments have the great advantage of being free from errors arising from the shifting or immobility of the index. It is not clear why their use was abandoned, except that they were difficult to construct and not generally known.

Six described his thermometer in 1782; but the first person to use it was Krusenstern, in 1803. It did not come into general use for deep-sea observations until the Arctic voyages of Ross and Parry, after which date it was, with the exception of Lenz's and Aimé's, employed for that purpose on all the expeditions sent out by foreign governments, as well as by our own. The necessity of protecting the instrument against pressure was early insisted upon by Lenz, Arago, Biot, and others; and there is reason to believe that protected thermometers were used by D'Urville and Bérard, for their observations in the same Mediterranean area show a remarkably close agreement with those recently made by Dr. Carpenter, with protected instruments, at and below depths of about 200 fathoms, the results being:—

D'Urville (May 1826).	Bérard (Nov. 1830).	Carpenter (Aug. 1870).
Surface....64°·1 F.	Surface....67°·1 F.	Surface....73°·5 F.
1062 ft.....54°·2	3189 ft.....55°·4	2958 ft.....55°·5
3189 ft.....54°·7	6377 ft.....55°·4	7968 ft.....54°·7

It was, however, on Du Petit Thouars's voyage of 1836 that the first special steps were taken to protect the thermometer against pressure. For that purpose an improved instrument of Bunten's was provided, and this was enclosed in a strong brass cylinder. Fifty-nine observations were made, of which Arago reported that 21 might be considered perfectly good. Temperatures of 36°, 37°, and 38° F. were recorded at depths (900 to 1100 *brasses*) in both the mid-Atlantic and mid-Pacific; while in one case, in taking a sounding at a depth of 12,271 feet near the equator in the Pacific, the instrument came up crushed, but with the index fixed at 34°·8 F. (1°·6 or 1°·7 C.). In a certain number of cases (24) the

pressure forced water into the cylinder. For these corrections were made.

In 1839, MM. Martins and Bravais made a series of observations in the sea between Norway and Spitzbergen with instruments carefully protected against pressure by means of glass tubes or metal cylinders. They used both self-registering thermometers (*thermométrographes*) and Walferdin's self-registering overflow thermometers, sending down two to four of each in every sounding, and taking the mean of the readings. These probably are amongst the most accurate observations on record. To a great extent they confirm those of Scoresby; and they further showed that the bottom-temperature near the Spitzbergen glaciers was about 29° F. None of the soundings exceeded 3000 feet.

In 1857, the late Admiral FitzRoy furnished Captain Pullen with thermometers specially constructed to resist pressure, and some very interesting, though somewhat variable results, were obtained therewith. On two occasions a temperature of 35° F. was recorded—one in the Atlantic, 26° 46′ S., at a depth of 16,200 feet, and the other in the Indian Ocean, at a depth of 13,980 feet.

With regard to the many observations made with unprotected instruments, they mostly admit of correction, which renders them available. Such corrections have been independently computed, with little difference, by Du Petit Thouars, Martins, Aimé, and the late Dr. Miller. The author, taking the mean of their estimates, uses as a coefficient −1° F. for every 1700 feet of depth.

In the third part of the paper the author shows the "State of the Question at the date of the Lightning Expedition." Ellis, Forster, Péron, and others early remarked on the decrease of temperature at depths in temperate and tropical seas, but it was not until 1823 that Lenz showed that a temperature of 35° to 36° existed at greater depths in those seas. Notwithstanding this, D'Urville in 1826, misled by incorrect readings obtained by previous observers with uncorrected instruments, and in the absence of sufficiently deep observations of his own, was led to believe that the temperature in open seas at and below a depth of 3214 feet (600 *brasses*) was nearly uniform at 39°·8 F. (4°·4 C.), and that between the latitudes of 40° and 60° there is a belt of a like nearly uniform temperature. A few years later, Arago, discussing the results obtained by Du Petit Thouars, insisted that they effectually disproved this hypothesis. Nevertheless, in 1839, Sir James Ross made the same mistake as D'Urville, and unfortunately obtained for it a wider circulation, which seems, however, to have been almost altogether restricted to this country. Still, Ross's numerous observations, when viewed under correction, are of considerable value, though the author considers that some error has occasionally crept into that uniform reading, so often recorded, of exactly 39°·5. Both D'Urville and Ross wrote under the opinion that sea-water, like fresh water, attained its maximum density at a tempera-

ture of between 39° and 40°,—a point that had been investigated and disproved by Marcet in 1819, approximately determined by Ermann in 1822, and which was finally settled by Despretz, in 1837, at 25°·4 F.

While the law of the decrease of temperature with the depth, in both the great oceans, to a point but little above the zero of Centigrade was being established, experiments had been carried on in polar seas showing, on the contrary, that the temperature at depths was higher than the average surface-temperature. The careful experiments of Scoresby and of Martins fully established this for the Arctic seas, and those of Ross, after correction, establish the same fact for the Antarctic Ocean. In one part, however, of the Arctic seas this rule has not been found to hold good; for, in Baffin's Bay, the experiments of John Ross, Sabine, and Parry, at depths of from 600 to 6000 feet, agree in showing a decrease of temperature of from 30° to 32° near the surface, to 29° and 28°·5 at the greatest depths attained. There are also two instances given of yet lower temperatures.

Nor were observations wanting in inland seas. Those of Saussure, D'Urville, and Bérard had indicated generally that, in the Mediterranean, the temperature decreased to a depth of about 1200 feet, after which it remained uniform at from 54° to 55° F.; and, in 1844, Aimé instituted a series of experiments which resulted in showing that the diurnal influence ceased to be sensible at a depth of from 16 to 18 metres, and the annual variation at a depth of from 300 to 400 metres, below which the temperature remained constant at 12°·6 C. (54°·6); and this he showed to be the mean winter temperature of the area of the Mediterranean, over which his observations extended. These observations were confirmed, for the Eastern Mediterranean, by those of Admiral Spratt. His first experiments in the Grecian archipelago showed, at a depth of 1200 feet, a temperature of 54°·5 to 55° F., while the later ones, at greater depths in the open sea, give, after correction, a temperature of about 55°. In the Red Sea, Captain Pullen found that while the surface-temperature varied from 77° to 86° F., it fell to 70° or 71° F. at 1200 to 1400 feet, below which it remained uniformly the same to the greatest depth he attained of 4068 feet. Some curious results were obtained in 1803–6 by Dr. Horner in the Sea of Okhotsh. The surface-temperature was 46°·4 F.; and the author finds (after correcting the original readings) at 360 feet a temperature of 28°, and at 690 feet of 28°·6, which is almost exactly that determined by Despretz as the temperature of sea-water at the moment of congelation.

The cause of the decrease of temperature with the depth in the great oceans was early investigated by physicists. Humboldt concluded that "the existence of these cold layers in low latitudes proves the existence of an undercurrent flowing from the poles to the equator." D'Aubuisson and Pouillet took the same view. D'Urville went further, and remarked that "it is rather a transport nearly in mass, and very slow, of the deep waters of high latitudes towards the equator," and that from his zone of

40° to 60° lat. there are two insensible currents—a lower one towards the equator, and an upper one towards the poles. Arago saw no other explanation than "the existence of submarine currents carrying to the equator the bottom waters of the icy seas."

We are, however, indebted to Lenz for a full and philosophical review of the whole subject in 1847. After showing that all the facts proved the existence of a temperature of from 34° to 35° F. at depths in the tropical seas, and that this could only be maintained by a constant slow under-current from the poles to the equator (which, on the other hand, must necessitate the transfer by an upper current of the equatorial waters to the poles), he proceeds to show by a series of observations, chiefly those of Kotzebue, and by a diagram, that a belt of cooler water existed at the equator, and that the temperature, at equal depths, was lower there than a few degrees to the north and south of it; and he concluded that this arose from the circumstance that the deep-seated polar waters there met and rose to the surface. As corroborating this view, he showed that the waters in the same zone were of lower specific gravity, a fact that had been before noticed by Humboldt.

The author then proceeds to consider some "General Conclusions." Some of these have now been better established by the more recent expeditions and by the researches of Dr. Carpenter. Taking, however, other areas, he shows that in the Arctic Ocean the bathymetrical isotherm of 35° is deepest on the west of Spitzbergen, while nearer Greenland and again nearer Norway the deep waters are colder. The several isothermal planes of 40°, 50°, 60°, 70°, and 80° are then traced southward, attaining their maximum depth between 50° and 40° lat., and rising thence towards the equator. Section No. 2, from Baffin's Bay to the equator, shows that the higher isotherms are not prolonged so far north as on the first line, and that the water at the bottom of the bay is colder than in the Spitzbergen seas, approaching much nearer that of its maximum density and of its point of congelation; whence he concludes that this is the main source of supply of the deep-seated cold waters in the Atlantic, which, after attaining their greatest depths between latitudes 40° to 50° N., are found 3000 to 4000 feet nearer the surface on approaching the equator.

In the South Atlantic, the bathymetrical isotherms show lesser curves; and while the isotherm of 40° crops out between the lat. of 50° and 55°, that of 35° is prolonged into high southern latitudes on a nearly uniform plane of 7000 to 8000 feet deep.

In the Pacific, the sections show that, notwithstanding there is no appreciable polar current through Behring's Straits, the bathymetrical isotherms of 60°, 50°, and 45° do not extend so far north as in the Atlantic, while that of 35° is apparently not prolonged beyond 60° N. lat. As the presence of temperatures lower than those which prevail in

parallel latitudes in the Atlantic cannot be due to north polar waters, and seems more than could be maintained by local influences, the author concludes that the effect may probably be due to waters from the Antarctic Ocean, of the presence of which the low temperatures at depths throughout the Pacific affords evidence, passing, in the absence of any counter flow, to the extremity of the North Pacific, where they are thrown upwards by the rising slopes of the ocean-bed, as on banks in open oceans. On the other hand, in the South Pacific the conditions seem very similar to those in the South Atlantic. The bathymetrical isotherms appear, however, to be prolonged further south than in the South Atlantic, which arises possibly from the circumstance that as none, or comparatively none, of the warm equatorial water can pass into the Arctic Ocean, a larger proportion passes into the Antarctic seas.

In the Southern and Indian Oceans the conditions seem analogous to those of the North Pacific, only they are more masked by the high surface-temperatures of the Arabian Gulf.

The author agrees in the opinion which has been advanced of the flow over the ocean-bottom of cold undercurrents at and below 35°, one from the north and the other from the south pole to the equator, and of their rise in the equatorial regions of the Atlantic. They must, then, necessarily tend to disperse and escape into other areas; but whether by a movement in mass of the upper strata, or by currents in more definite channels, or by both causes combined, remains to be proved by further research. He inclines to the latter view. He would suggest the question whether the Gulf-stream, together with others which seem to originate or acquire additional power in equatorial seas, such as the Guinea and Brazilian currents, may not receive either their initial start or be strengthened and maintained by the surging-up of the Arctic and Antarctic waters at the equator, while another portion of those waters may be deflected back in insensible currents to polar regions. In the same way some of the great currents of the North Pacific may arise.

The paper concludes by a review of the other causes connected with these conditions, by a consideration of the normal isotherms of the polar regions, and by a comparison of the temperatures of inland seas, which are dependent on local climatal conditions, with those of the great oceans, which are subject to such vast distant influences; and he directs attention to the important bearing which these questions of oceanic physics have on many geological problems.

XXIII. "On the Sun-spot Period and the Rainfall." By J. A. Broun, F.R.S.

Having read with much interest Mr. Meldrum's communication to the Royal Society on the apparent simultaneity of excess of rainfall and sun-spot area*, I have waited some confirmation of his conclusions from a more extensive induction. Mr. Hennessey's "Note" in the Proceedings of the Society for April 1874† induces me to offer the following views and results to the Royal Society.

It is well known that the amount of rainfall is a very variable quantity in some countries and in certain positions, and that when there is a year of drought in one part of the world, there is frequently an excess of rain in another. Any investigation, then, which should be occupied with the average fall of rain over the earth's surface must be long and laborious, unless the variation to be dealt with is large and marked compared with others which must be considered purely accidental relatively to the sun's spots. In proof of this I may cite the rainfall at Mussoorie given by Mr. Hennessey‡, where, as far as the sun-spot area is known, any result favourable to the connexion of the two phenomena depends wholly on the rainfall for 1861, which is upwards of 50 inches in excess of the mean. If this excess be not due to the great spot-area, then a long series of years' observations might be requisite to make the positive and negative errors destroy each other.

It has been with the intention of determining what may be the effect of a given change of sun-spot area, within a limited district, during a period favourable to the connexion of the two phenomena, that the following discussions have been made. We can then say approximately within what limits the excess and deficiency of rainfall lie for the years of greatest and least spot-area, what amount of observations may be required to destroy accidental variations, and whether the result may encourage more extensive research.

Mr. Meldrum finds a *mean* difference of 8·5 inches of rain between the falls for the years of greatest and least spot-area§; but this result is derived to some extent from short series of observations made in different parts of the world, and gives no weight to the rainfall in other years than those considered years of maximum or minimum sun-spots.

Should there be any connexion betwixt the rainfall and spot-area, we may always in the first instance represent it approximately by an equation of this form,

$$\Delta R = f \Delta A,$$

where ΔR is the excess or deficiency of the rainfall from the mean, ΔA

* Proceedings of the Royal Society, vol. xxi. p. 297.
† *Ibid.* vol. xxii. p. 286.
‡ *Ibid.* vol. xxii. p. 287.
§ *Ibid.* vol xxi. p 305.

is the excess or defect of spot-area for the same period of time, and f is a constant to be deduced from the observations.

Having obtained the mean spot-area for each year from 1832 to 1867, from Table VII. of the paper on this subject by Messrs. De La Rue, Stewart, and Loewy*, the mean for three periods of 11 years (1832 to 1864) was found equal to 643 millionths of the sun's visible surface; with this quantity the values of $\pm\Delta A$ (in millionths of the sun's surface) for each year were obtained.

Mr. Meldrum's conclusion depends chiefly on observations during these periods in Great Britain; and as he has deduced the rainfall for the first period of minimum spots from observations at three stations, Greenwich, Carbeth (near Glasgow), and Aberdeen, I first examined the observations at these places together with simultaneous observations at Makerstoun for the two periods 1832 to 1853†. Applying the above equation to these observations, the following results were obtained:—

Greenwich ...... $\Delta R = -0{\cdot}00092\ \Delta A$;
Makerstoun .... $\Delta R = -0{\cdot}00020\ \Delta A$;
Carbeth .... $\Delta R = +0{\cdot}00158\ \Delta A$;
Aberdeen ...... $\Delta R = +0{\cdot}00128\ \Delta A$.

Greenwich and Makerstoun are thus opposed to the conclusion, and Carbeth and Aberdeen are more strongly in its favour. It should be remarked, however, that the result for Aberdeen depends wholly on the rainfall given for that place in 1834 (12·3 in.) being exact. As it is 12 inches less than the mean, while at the other three stations the deficiency is only from 0·6 in. at Greenwich and Makerstoun to 1·2 in. at Carbeth, this may be due to a leaky rain-gauge or to a clerical error of 10 inches. In any case no great weight can be given to the conclusion from these four stations‡.

I now sought for an approximation to the mean fall of rain for Great Britain, and for this end have employed the quantities deduced by Mr. Symons from ten stations (British Association Report, 1865, p. 203; 1871, p. 102). The differences of spot-area from the mean, in millionths of the sun's surface, and of the rainfall for each year are given in the following Table:—

* Phil. Trans. 1870, p. 399.

† The means for Makerstoun during the years 1832 to 1849 will be found in Trans. Roy. Soc. Edinb. vol. xix. pt. ii. p. 108; the falls for the other years are—1850, 21·49 in.; 1851, 25·57 in.; 1852, 32·20 in.; 1853, 23·54 in.

‡ It may here be noted that the sum of the *plus* and *minus* differences of R and the mean rainfall for the four stations during the twenty-two years were—

	Greenwich.	Makerstoun.	Carbeth.	Aberdeen.
Mean fall	24·4 in.	26·2 in.	43·6 in.	24·2 in.
Sums of ΔR......	100·1 in.	67·8 in.	92·4 in.	94·3 in.

It will be seen that the sums of differences have no relation to the mean fall of rain.

Differences of Rainfall for Great Britain and of Sun-spot area for 1832 to 1867.

Year.	ΔA.	ΔR.	Year.	ΔA.	ΔR.	Year.	ΔA.	ΔR.	Means. ΔA.	Means. ΔR.
		in.			in.			in.		in.
1832.	−359	−1·54	1843.	−540	+2·66*	1854.	−501	−5·36	−467	−1·41
1833.	−558	+1·97*	1844.	−465	−4·02	1855.	−566	−4·47	−540	−2·17
1834.	−506	−3·22	1845.	−232	+0·13*	1856.	−619	−1·85	−452	−1·64
1835.	+171	+0·82	1846.	− 5	+1·83*	1857.	−428	−2·04	− 87	+0·20
1836.	+746	+5·75	1847.	+469	−1·94*	1858.	+177	−4·95*	+444	−0·38
1837.	+556	−3·20*	1848.	+395	+8·24	1859.	+756	+0·79	+569	+1·94
1838.	+293	−0·63*	1849.	+203	+0·77	1860.	+656	+5·60	+384	+1·91
1839.	+164	+3·53	1850.	−123	−1·39	1861.	+659	−0·76*	+267	+0·46
1840.	− 46	−3·07	1851.	+ 40	−1·04*	1862.	+530	+2·63	+174	−0·49
1841.	−306	+5·77*	1852.	− 92	+7·79*	1863.	− 15	−0·81	−138	+4·26
1842.	−429	−2·21	1853.	−253	−0·36	1864.	+245	−5·63*	−146	−2·73
						1865.	−187	+1·90*	−409	−0·27
						1866.	−342	+3·26*	−458	−1·74
						1867.	−468	+0·70*	−440	−0·34

If we seek the value of *f* for the mean of the three periods of eleven years commencing 1832 and 1835, we find the following equations:—

1832 to 1864 .... $\Delta R = +0{\cdot}0019\ \Delta A$;
1835 to 1867 .... $\Delta R = +0{\cdot}0011\ \Delta A$.

These results, then, are, as we expected, in conformity with Mr. Meldrum's conclusion; so that if we compare the year of largest with that of smallest spot-area, the difference of rainfall should amount to 2·61 in. by the first and to 1·51 in. by the second value of *f*. If we take the mean spot-area for the years 1834, 1844, 1856, and 1866, and for 1836, 1848, and 1861, we find that the mean difference of rainfall for these years should be 2·06 in. by the first and 1·20 in. by the second value of *f*, instead of 8·45 in. as found by Mr. Meldrum.

It will be seen also that the greatest mean difference of rainfall is that for the years 1841, 1852, and 1863, and this was an excess of rain for years of spot-area deficiency; were another such opposite difference to present itself, it would neutralize the conclusion derived from these means. It should also be observed that while the first and third periods of eleven years are in favour of the connexion, the second (1843 to 1853) is opposed to it (this is also the case for the eleven years 1857 to 1867).

It will be seen, then, that from this discussion a probable difference of about 2 inches of rain may be expected betwixt years of greatest and least spot-area.

This result is derived from observations at ten stations, distributed over a very small patch of the earth's surface; and it is evident that for any serious investigation a much larger series of observations representing the rainfall over a great extent of country would be essential.

* Indicates opposite signs of ΔA and ΔR.

I now examined observations made at different stations in India; but this examination showed the extreme difficulty of obtaining a satisfactory result, either way, from a few stations in that country, when, in certain years, the accidental excess of rainfall at some of the stations may be 40 inches, even though deficiencies at some stations may diminish the amount of the error.

From my own experience of rainfall on the Indian ghats, I should doubt that a mountain-station, such as Mussoorie, is well fitted to be employed in this discussion. If a single station could be taken to represent any tract of country, it ought to be one least liable to local causes of variation. Among the mountains a slight change in the average direction of the wind will cause great differences in the rainfall at stations but little distant from each other, and to eliminate accidental variations of 40 or 80 inches would require observations during a very long series of years.

The following Table will, however, show the quantities which may have to be dealt with at an Indian hill-station †:—

Values of ΔR for Mahabuleshwar, 4500 feet above the sea, with the corresponding values of ΔA.

Year.	ΔA.	ΔR.	Year.	ΔA.	ΔR.
		in.			in.
1832.	−359	−26·1	1843.	−540	+32·7*
1833.	−558	−49·3	1844.	−465	+ 9·3*
1834.	−506	+44·3*	1845.	−232	− 3·1
1835.	+171	−26·3*	1846.	− 5	+35·3*
1836.	+746	− 9·4*	1847.	+469	−34·2*
1837.	+556	+14·8	1848.	+395	− 8·0*
1838.	+293	−72·8*	1849.	+203	+85·4
1839.	+164	−19·8*			
1840.	− 46	+31·4*			
1841.	−306	+28·0*			
1842.	−429	+51·9*			

From this Table we derive the equation

$$\Delta R = -0{\cdot}02 \text{ in. } \Delta A,$$

or that 26 inches *more* rain falls for the year of *least* than for that of greatest spot-area. The examination of many series of observations has shown how difficult it will be to arrive at a conclusion for a quantity so small as 2 inches of rain.

It is evident that a larger tract of country than Great Britain should be chosen, and the approximate rainfall be deduced from the greatest

† For the rainfall at Mahabuleshwar, see Colonel Sykes's paper on Indian observations, Phil. Trans. 1850, p. 367. The mean fall is 253·0 inches.

* Indicates opposite signs of ΔA and ΔR.

possible number of stations. Germany and France may give sufficient data for such a trial. Were the result well marked, there would be reason to seek for its confirmation in other countries; but to undertake this labour, better grounds, I think, must be found than I have hitherto been able to obtain. The admirable series of observations which Mr. Symons is obtaining will suffice for the future, as for the past, ten years to give a very near approximation to the excess or deficiency of rainfall in Great Britain.

XXIV. "On the Mechanism of Stromboli." By ROBERT MALLET, M.A., F.R.S. Received May 17, 1874*.

The Society then adjourned over the Long Vacation, to Thursday, November 19.

Presents received May 21, 1874.

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Photographs of Absorption-spectra of Didymiums and other Solutions.
W. N. Hartley.

"On the Absorption of Carbonic Acid by Saline Solutions."

By J. Y. Buchanan, Chemist on board H.M.S. 'Challenger.'

Communicated by Professor A. W. Williamson, For. Sec. R.S. Received December 11, 1873 *.

In the examination of sea-water, whether it be regarded from a chemical or from a zoological point of view, the determination of and the variations in the amount of carbonic acid in different parts of ocean must always be an object of importance. This is more especially so when a parallel series of observations on the quantity of oxygen present is carried out. At the surface we should expect to find the quantities of these gases following the law of partial pressures; at greater depths, however, where the water for long periods only comes in contact with water, we should expect to find the quantity of oxygen decreasing and that of carbonic acid increasing with the amount of animal life. The investigation from this point of view of the bottom-water, at greater and smaller depths, presents perhaps a more interesting field of observation than that of intermediate depths. Down to nearly 2000 fathoms life is still abundant; below this depth, however, the amount rapidly decreases till, at about 2800 fathoms, it is, for carbonic-acid producing purposes, practically extinct. We have, then, to settle the variation of the carbonic acid with latitude and longitude, with depth, with nature of bottom, and with nature of atmosphere.

In order to solve these problems, it is before all necessary to have a reliable method for the determination of the carbonic acid. For the discovery of a cause of error in the old method, and for the invention of a new one, we are indebted to Dr. Jacobsen, of Kiel. Dr. Jacobsen found that sea-water could not, as had been till then assumed, be thoroughly freed from its dissolved carbonic acid by merely boiling *in vacuo* †. He found that it was necessary to boil down almost to dryness before the last traces of carbonic acid could be expelled. Being particularly interested in the matter, I immediately commenced a series of experiments to determine, if possible, the salt or salts to which sea-water owes this peculiar property. A short *résumé* of the results of these experiments have been published as an appendix to Professor Wyville Thomson's 'Depths of the Sea.'

I purpose here to give a detailed account of the experiments performed. They consisted of two series—the one analytical, the other synthetical. In the former I was ably assisted by Mr. George Macdougald, in the latter by Mr. Robert Romanes, junior assistant in Professor Crum Brown's

* Read February 19, 1874. See *antè*, p. 192.

† Dr. Andrews informs me that he had observed a similar phenomenon when attempting to determine the amount of atmospheric gases in sea-water, by boiling it in the Torricellian vacuum after the manner of vapour-density determinations.

laboratory in the University of Edinburgh, and I gladly avail myself of this opportunity to thank them both.

The analytical series consisted of experiments on solutions of the different salts saturated with carbonic acid. A certain quantity of each was distilled almost to dryness, the steam being condensed in an ordinary Liebig's condenser, to which was fitted a tubulated receiver, having a bulbed V-tube attached containing baryta-water. The distillation was interrupted and the baryta-water changed after the passage of every eighth of the distillate, the amount of carbonic acid passed being roughly estimated by the apparent turbidity of the baryta-water. The object of these experiments being to find out which of a number of saline solutions had the property of retaining carbonic acid, and to ascertain roughly what length of time one must boil in order thoroughly to expel it, an accurate determination of the carbonic acid actually passing during the intervals would have been superfluous. Besides these, a number of quantitative determinations were made of the amount of carbonic acid actually absorbed by different solutions.

The synthetical series consisted of experiments for the determination of the absorption-coefficients of two solutions—the one of sulphate of magnesia, the other of sulphate of lime.

Let us take the analytical series first. As before remarked, it is subdivided into two sets, which we shall treat in their order. In the one observations were made on the elimination of the carbonic acid as the distillation proceeded; in the other an attempt was made to determine how much carbonic acid, in a saline solution saturated with the gas, was actually retained or bound, or at least kept from freely exercising its properties as a gas, by the presence of the salt in the solution.

First Experiment.—In order to have a certain standard of comparison in judging the retardation caused by salts in the escape of carbonic acid from solutions on boiling, distilled water was saturated with the gas and distilled in the manner indicated above. During the passage of the first eighth of distillate the gas evolution was of course abundant, during the second a perceptible quantity passed, after which no more could be detected. It may be assumed, then, that, in the experiments which followed, the carbonic acid held *simply in solution* by the water passes almost entirely in the first eighth part of the distillate, and that whatever passes afterwards has been retained, in some way or other, by the salt in solution. In conducting these experiments no baryta-water was put in the receiver itself, but only in the V-tube. The water collected was always tested with baryta-water, and with the general result that in the first fraction carbonic acid was present in abundance, while in the latter ones there was rarely a trace to be detected. That the distillate consisting of pure water should contain not a trace of the gas, whose presence in the atmosphere above it is attested by the precipitate

in the exit-tube, shows that, at feeble pressures, the solution of carbonic acid requires considerable time for its completion.

Second Experiment.—A chloride-of-sodium solution containing 2 per cent. Na Cl had a strong stream of CO_2 passed through it for about ten minutes and was then distilled. During the passage of the second* eighth there was still a considerable quantity, during that of the third a very slight trace, and after that none.

Third Experiment.—A chloride-of-magnesium solution containing 0·25 per cent. $Mg\ Cl_2$ was treated in the same way as the chloride-of-sodium solution, when the *whole* of the CO_2 passed in the first fraction.

Fourth Experiment.—A solution containing 4 per cent. $Mg\ Cl_2$ and 10 per cent. Na Cl was saturated with CO_2 and allowed to stand in a closed vessel over night. On distilling, it was found that carbonic acid continued to be given off in perceptible but gradually decreasing quantities until the end.

Fifth Experiment.—For this and the three following experiments a solution containing 12·3 grammes crystallized sulphate of magnesia (in a litre?) was used. Carbonic acid was passed through some of this solution for about 10 minutes, and the liquid allowed to stand in a closed vessel over night. On distillation there passed during the second fraction very little, during the third and fourth fractions decidedly more, during the fifth again much less, and afterwards, to the end, slight but perceptible traces of carbonic acid.

Sixth Experiment.—The solution was heated to nearly boiling, and CO_2 passed into it until it was cold, the whole being allowed to stand over night. During the passage of the second, third, fourth, and fifth fractions, the amount of gas was about constant and small. It increased greatly during the sixth, falling away again during the seventh.

Seventh Experiment.—The conditions were exactly the same as those of the fifth experiment; and the results in the two cases agreed well with each other, the amount of gas coming off increasing slightly about the middle of the operation. In order to see if the rise of temperature consequent on concentration had any thing to do with the phenomena under consideration, a thermometer was immersed in the boiling liquid. It read at the end of the second, third, fourth, and fifth fractions 102°·5, 102°·9, 103°, and 103°·2 respectively.

Eighth Experiment.—The conditions were the same as in the last, only that the solution stood two days before distillation. In the results there was this peculiarity, that in the fourth fraction the carbonic acid disappeared altogether, reappearing, however, again in the fifth.

* It is unnecessary in the cases where no means were taken to free the solution from simply dissolved carbonic acid to repeat in each one that the first fraction contained abundance.

Ninth Experiment.—Sea-water from the Firth of Forth was distilled. Here, as in the case of the sulphate of magnesia, the amount coming off increased about the middle, falling away again in the fifth fraction. In the sixth, however, it experienced a slight increase, falling off again towards the end of the operation.

From these experiments we may conclude that, alone and in the degree of concentration in which they occur in the sea, the two most abundant salts, namely chloride of magnesium and chloride of sodium, exercise no retarding influence on the liberation of carbonic acid on boiling. When mixed, however, as in the fourth experiment, they appear to have this effect. Whether, if sufficiently diluted to represent sea-water, they would continue to do so I was unable to ascertain, as the investigation of the sulphates occupied all the time at my disposal. It is further evident that, in the sulphate-of-magnesia solution experimented on, we have a solution which behaves towards carbonic acid in the same way as sea-water.

Let us pass now to the second set of the analytical series—namely, the estimation of the amount of carbonic acid retained in consequence of the presence of the salts in question. The apparatus used was the same as that in the last set, baryta-water being contained both in the receiver and in the V-tube.

Experiments were made on solutions of sulphate of magnesia, of sulphate of magnesia and chloride of sodium, and of sulphate of lime, to which were added some on sea-water itself. In every experiment the quantity of solution operated on was 300 c. c. The carbonic acid coming off was retained by baryta-water of known strength, the remaining free baryta being afterwards determined by means of oxalic acid. Rosolic acid was used to determine the point of neutralization. The oxalic acid was rather stronger than tenth-normal; it contained 6·478 grms. $C_2 H_2 O_4 + 2H_2 O$ in the litre, which is equivalent to 2·259 grms. carbonic acid. 1000 c. c. baryta-water required 3235 c. c. oxalic acid for neutralization.

The method of conducting the operation was as follows:—Carbonic acid was passed through the solution until it could be assumed to be saturated. Six to seven litres of air were then drawn through it cold, after which it was heated to boiling, and allowed to boil for from two to three minutes in a current of air. The receiver with the baryta-solution was then attached, and the distillation continued in a stream of air, until the contents of the flask were nearly dry. The baryta-water then remaining unneutralized was titrated, and from it the amount of carbonic acid ascertained.

Experiments on sulphate-of-magnesia solution containing 12·3 *grammes crystallized salt per litre.*—As all were conducted in precisely the same way, it will be sufficient to give the results in a tabular form. The first

three experiments were made with portions of one and the same solution; for the last two a fresh solution (prepared, to all appearance, in exactly the same way as the previous one) was used. The difference in the results obtained show the precarious nature of the combination:—

Volume of solution used.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 c. c.	Grammes carbonic acid in 1 litre.
300 c. c.	25 c. c.	78·96 c. c.	0·0043	0·0143
300 „	10 „	30·0 „	0·0053	0·0165
300 „	10 „	30·9 „	0·0033	0·0110
300 „	15 „	47·5 „	0·0023	0·0077
300 „	10 „	31·32 „	0·0023	0·0077

Two experiments were made with a solution prepared as follows:— The quantity of sulphuric acid necessary for the formation of 12·3 grammes crystallized sulphate of magnesia was diluted to a litre, and pulverized carbonate of magnesia suspended in it. Although the mixture was allowed to stand over night, shut off from the influence of the atmosphere, the solution was still exceedingly acid. It is well known that carbonate of magnesia is difficultly soluble in cold dilute acids. To have heated the solution would have frustrated the object of the experiment, which was, by bringing nascent sulphate of magnesia together with nascent carbonic acid at ordinary temperatures, to give them the best opportunity of combining.

Two experiments were made with a similarly prepared solution of sulphate of lime. In this case sulphuric acid was added to the water in quantity sufficient to form, with lime, more salt than was necessary for the production of a saturated solution of gypsum. Here neutralization took place without difficulty, and, as might have been expected, the amount of carbonic acid formed was considerably greater than in the case of the magnesia salt.

Two experiments were made with an ordinary sulphate-of-magnesia solution containing 2·05 grammes crystallized salt per litre.

Two further experiments were made with a solution containing 2·05 grammes sulphate of magnesia and 20 grammes chloride of sodium per litre. All were conducted in the way described above, and the results are given in the following Table. The experiments with the carbonates of magnesia and lime were made at a considerably later date than the others; the value of 10 c. c. baryta-water had in consequence become equivalent to 32 c. c. instead of 32·34 c. c. oxalic acid.

Nature of solution.	Volume of solution used.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 c. c.	Grammes carbonic acid in 1 litre.
$MgCO_3$ + H_2SO_4	300 c. c.	10 c. c.	30·6 c. c.	0·0032	0·0107
	300 „	10 „	30·9 „	0·0025	0·0083
$CaCO_3$ + H_2SO_4	300 „	10 „	27·5 „	0·1014	0·3380
	300 „	10 „	27·5 „	0·1014	0·3380
2·05 grms. $MgSO_4+7H_2O$ per litre.	300 „	10 „	31·2 „	0·0026	0·0087
	300 „	10 „	31·3 „	0·0023	0·0077
$MgSO_4+7H_2O$ + NaCl	300 „	10 „	31·6 „	0·0016	0·0053
	300 „	10 „	31·4 „	0·0021	0·0070

Five experiments were made with sea-water taken at the end of Portobello Pier, on the Firth of Forth. In the first three it was submitted immediately to the same treatment as the saline solutions; in the last two carbonic acid was first passed through it for some time. As the results given in the following Table are identical, it is evident that, in its natural state, the water in question was saturated with carbonic acid in this peculiar state of combination.

Experiments on Sea-water.

Volume of water used.	Volume of baryta-water.	Volume of oxalic acid.	Grammes carbonic acid in 300 c. c.	Grammes carbonic acid in 1 litre.
300 c. c.	15 c. c.	39·75 c. c.	0·0198	0·0660
300 „	10 „	23·0 „	0·0211	0·0703
300 „	10 „	23·15 „	0 0208	0·0693
300 „	10 „	23·34 „	0·0203	0·0677
300 „	10 „	23·34 „	0·0203	0·0677

Subsequent experiments made at sea, on water from mid-ocean and from various depths, have shown me that the above quantities are very much in excess of the quantities usually contained in ocean-water. From the large quantity of organic matter poured into the Forth, not far from Portobello, there must be an abundant production of carbonic acid in the water itself, and we have seen above the effect of bringing sulphate of lime and carbonic together in the nascent state. Sea-water contains, on an average, about 8 parts sulphate of lime in 10,000. A saturated solution of the same salt in distilled water contains at 15° C. 24 parts in 10,000.

Under the most favourable circumstances, then, one would expect sea-water to bind about one third of the quantity retained by an equal volume of sulphate-of-lime solution. We have seen that a litre of this solution is capable of retaining 0·338 grm. CO_2, while the same volume of sea-water contained only 0·07 grm., or considerably less than the third of that held by the sulphate of lime. In ocean-water I have never yet found more than 0·064 grm. CO_2 per litre, including both the *simply dissolved* and the *half bound*. We have, then, in the sulphate of lime alone, an agent capable of retaining much more carbonic acid than is usually found to exist in sea-water. Besides this there is also, at least, the sulphate of magnesia possessing this property. How much it would be capable of absorbing if the carbonic acid were presented in a nascent state in a neutral solution we do not know; it would be interesting to determine the amount of carbonic acid retained by a sulphate-of-magnesia solution in which organic matter had been allowed to decay.

The practical conclusion to be drawn from the preceding experiments is that, as the carbonic acid is retained by the presence of certain sulphates, the gas will be more easily boiled out if we get quit of these sulphates. For this purpose I always add to the sample of sea-water, in which the CO_2 is to be determined, a sufficient quantity of a saturated chloride-of-barium solution to precipitate all the sulphuric acid present. The effect has answered my expectations. After the first fifth of distillate has passed, there is rarely a perceptible turbidity in fresh baryta-water. In practice, however, and as it costs but little trouble, I always distil off from three quarters to seven eighths, and often quite nine tenths of the solution.

The determination of the carbonic acid in sea-water is carried on on board the 'Challenger' by means of an apparatus, a very slightly modified form of the one described by Dr. Jacobsen in the 'Annalen der Chemie und Pharmacie,' a drawing and description of which he was good enough to give me when the 'Challenger' was fitting out.

A flask with a capacity of about 500 c. c. receives the sea-water to be operated on, usually from 200 c. c. to 250 c. c. It is closed by an india-rubber cork, through which pass two tubes; one, reaching to the bottom, communicates with the condenser, a cylindrical copper vessel, 10 in. high by 5½ in. diameter, with a block-tin worm. The lower end of the worm is attached to the receiver by a bent glass tube with a flexible joint, from which a glass tube leads to the bottom of the receiver. The flexibility thus obtained is, in practice, of the greatest use, enabling the operator, by shaking, to expose constantly fresh surfaces of baryta-water to the passing gases. The receiver is connected by an india-rubber tube with two bulbed V-tubes. An aspirator enables a stream of air to be drawn through the apparatus, a soda-lime safety-tube being interposed between it and the V-tubes. The water running from the aspirator is conducted outside the port by a tube which passes

through a hole in the sash. The flask containing the sea-water is supported on a ring, by a clasp holding its neck. Both of these, along with the spirit-lamp underneath the flask, are attached in the usual way to an iron rod, which is attached to the projecting side of the ship by an eye-bolt, in which it has a play of rather more than an inch in the direction of its length. The lower end of the rod sits securely in a hole, let into the top of the working-table. When the apparatus is dismounted the rod is pushed up, till its lower end has freed itself from the hole and laid flat along the roof, being supported at one end by an eye-bolt, at the other by a hook. The aspirator and the condenser are retained in their places by wooden blocks, which fit in between them and the ship's side or the battens on the bench.

The water in which the carbonic acid is to be determined is introduced into the flask by means of a tube reaching to the bottom. When the carbonic acid is to be determined in a specimen of water, the apparatus is first put together and a current of air, free from carbonic acid, drawn through, care having been taken to see that it is thoroughly dry in all its parts. The corks in the receiver and V-tubes are then eased, and from 15 c. c. to 20 c. c. baryta-water, usually of about $\frac{1}{10}$ normal strength, run into them. The water to be examined is introduced into the flask through a tube reaching to the bottom; 10 c. c. of a nearly saturated solution of chloride of barium are then added, the apparatus closed and heat applied. When the liquid begins to boil, care must be taken to lower the flame to avoid frothing over. A gentle current of air is now conducted through the boiling liquid, and the receiver constantly agitated. After half an hour's boiling, about 100 c. c. water have distilled over, and at the same time *all* the carbonic acid. That the latter is the case, I ascertained by changing the baryta-water at this point, and continuing the distillation, when no turbidity was produced. That, at any rate, no appreciable amount of carbonic acid passes after even the first 50 c. c. water have been distilled over, may be very easily seen by the liquid in the receiver passing from a turbid, somewhat frothy solution, to a clear one, in which a well-defined precipitate is suspended, and whose amount does not visibly alter as the distillation proceeds. Although such is the case, I have usually, as it costs but little more time and trouble, carried on the distillation until seven eighths have passed, and indeed, in many cases, until crystallization has commenced. When proper attention is paid to the agitation of the receiver during the first part of the distillation, the amount of carbonic acid reaching the first V-tube is quite insignificant, and the baryta-water in the second remains perfectly clear. When this operation is finished, the contents of the V-tubes are washed into the receiver with boiled water, and the remaining alkalinity determined with hydrochloric acid of known and convenient strength. The point of neutralization is indicated by rosolic acid.

Synthetical Experiments.—In order to check the above experiments, it appeared to me to be of importance to determine the absorption-coefficients of one or more of the solutions for carbonic acid. Before knowing any thing of the retention of carbonic acid by sea-water, I had determined, if possible before the sailing of the 'Challenger,' to investigate the solutions of some of the salts occurring in sea-water, with reference to their power of absorbing the atmospheric gases. It was well known that sea-water, in common with most other salt-solutions, absorbed a smaller quantity of air than distilled water would do under the same circumstances; but it had never, to my knowledge, been attempted to find out whether this diminution of absorptive power was distributed equally over the three gases, or was exhibited more strongly in the case of one gas than in that of another. From a preliminary experiment in which a 2 per cent. solution of Na Cl in distilled water was saturated with air and the air then expelled and analyzed, it appeared that the oxygen was present in slightly greater quantity relatively to the nitrogen than would have been the case if the liquid had been distilled water. Of course, this being the result of only one experiment, no conclusion can be drawn from it; but it shows the necessity of the investigations which I had proposed to myself. Unfortunately, the time at my disposal was too short to allow of any thing being done, except a few experiments with carbonic acid and solutions of sulphate of magnesia and of sulphate of lime.

For this purpose I made use of a Bunsen's absorptiometer, and followed his method, with the modifications rendered necessary by having to do, not with a simple liquid, but with a saline solution. The carbonic acid was introduced into the absorption-tube and measured, not in the mercurial trough, but in the absorptiometer itself, the lid being left open. This is a much more expeditious way, inasmuch as the gas quickly assumes the temperature of the water of the absorptiometer; and as the readings after absorption are all done in this way, there can be no object in reading the gas alone in another way. After absorption, the instrument is always read with the lid shut, so that what corresponds to the height of mercury in the trough is given by the height of it in the outside graduated leg of the absorptiometer. In all of the determinations this height is given, not in the reading on the leg itself, but in the corresponding reading on the absorption-tube, which can be directly observed with sufficient accuracy with the ordinary telescope used in gas-analysis. As after shaking the instrument and opening the stopcock connecting the leg with the body of the instrument some of the water frequently passed into the former, we have generally a reading marked "water in outer leg," which forms a factor in estimating the tension of the gas.

The solutions experimented on were, one containing 1·23 per cent. crystallized sulphate of magnesia and one containing 0·205 per cent. gypsum. It was necessary that these solutions, before being introduced

into the absorption-tube, should be deprived of air without affecting their state of concentration. This was effected in the following way. A flask was filled up to a mark in the neck with the solution, half of it was then emptied into another flask and the two boiled, while distilled water was kept boiling in a third. When the boiling had been kept up for about half an hour, the contents of the second flask were emptied into the first and washed out with the hot distilled water, the volume of the solution being brought in this way up to the mark, and so far above it as was equivalent to the expansion of the solution for the difference of temperatures. A glass syringe of convenient size was now filled with the boiling liquid and passed hot into the absorption-tube. This method may be objected to on two grounds: first, that there is some uncertainty about the exact concentration of the solution when introduced into the absorption-tube; and, second, that some air may have been absorbed by the liquid in its passage through the syringe to the tube. As to the first objection, when the operation is carried out in the way I have described, the possible difference between the actual and assumed concentration is so small that it would be extremely unlikely to have any influence on the coefficient of absorption of the liquid. As to the second, if the manipulations be expeditiously carried out, there is but little fear that the liquid at so high a temperature, and exposed to the small quantity of air of diminished tension in the syringe, should be contaminated in an appreciable way. However much or little importance one may attach to these possible sources of error, they probably explain why the whole subject has been left almost entirely untouched.

Our object in these experiments is, not to determine the absorption-coefficient for a standard pressure such as 760 millims., but to determine it for various pressures, the temperature being kept as uniform as circumstances will permit, and to compare the results obtained with those calculated for distilled water.

Let V = volume of gas (at 0° and 760 millims.) before introduction of solution,

V_1 = volume (reduced to 0°) of gas after absorption,

P_1 = pressure of this gas,

then the volume of gas absorbed will be

$$V_2 = V - \frac{V_1 P_1}{760} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

And if h be the volume of the solution, we have for the coefficient of absorption at pressure P_1 and the temperature of observation,

$$a_1 = \frac{1}{h_1}\left(V - \frac{V_1 P_1}{760}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Two series of experiments were made on solution of sulphate of magnesia containing 1·23 per cent. crystallized salt, and one series on sulphate-of-lime solution containing 0·205 per cent. of $CaSO_4 + 2H_2O$.

The detailed results are given in four Tables. In the first are given the original volumes of the carbonic acid used in each series, with the data for finding the same. In columns 1 and 2 are found the volumes used in series I. and II. respectively for sulphate of magnesia; in column 3 is the volume for the sulphate-of-lime experiments.

TABLE I.

Table giving the original volumes of carbonic acid used in the three series of experiments I., II., and III.

		1.	2.	3.
Barometer	δ	741·5	741·5	736·5
Lower mercury-level in trough	a_1	594	581	586
Upper mercury-level in tube	b_1	323	322	331
Mercury column in tube (δ_1)	a_1-b_1	271	259	255
Height of water column in outer cylinder	w_1	552	544	558
Equivalent mercury column	q	40·7	40·15	41·18
Temperature of water in cylinder	t	10·8	9·8	10·8
Vapour-tension for $t°$	T	9·665	9·045	9·665
Reacting pressure ($\delta-\delta_1+q-$T)	P	501·5	513·6	513
Volume in c. c. of b_1 from calibration Table	v	73·267	73·039	75·09
Volume (v) reduced to 0° and 760 millims.	V	46·509	47·650	48·759

In series III. (Table IV.) with sulphate of lime, and in series I. (Table II.) with sulphate of magnesia, the readings were made without loss of time, the pressure being successively increased for each experiment. In series II. (Table III.) particular attention was paid to the length of time this gas and solution were in contact at the different pressures, and for each experiment the pressure was successively diminished. They were left in contact for nine days at the highest pressure (column 1) before reading; No. 2 was read twenty-two hours later, No. 3 forty-one hours later than No. 2, No. 4 twenty-five hours later than No. 3, and No. 5 after the lapse of some days.

TABLE II.

Determinations of the absorption-coefficient of a 1·23 per cent. solution of crystallized sulphate of magnesia for carbonic acid at the temperatures and pressures indicated. V=46·509 c. c.

		1.	2.	3.	4.
Barometer	δ	741·5	739·5	741·5	738·0
Thermometer	t	12·0	11·5	11·9	11·9
Outer level of mercury	a_1	568·7	293·0	397·5	182·5
Inner level in absorption-tube	b_1	314·2	224·0	255·5	199·0
Upper level of solution in absorption-tube	c_1	162·0	69·0	102·0	39·5
Upper level of water in outer leg	f_1	568·7	242·0	352·0	123·0
Resultant water column $\{a_1-f_1-b_1+c_1\}$	W	−152·2	−104·0	−108·0	−100·0

TABLE II. (*continued.*)

		1.	2.	3.	4.
Equivalent mercury column	q	− 11·2	− 7·68	− 7·97	− 7·38
Vapour-tension for $t°$.........	T	10·457	10·120	10·389	10·389
Resultant pressure on gas $\{\delta-\delta_1{}^*-\mathrm{T}+q\}$	P_1	476·54	652·7	581·14	736·73
Volume in c. c. of c_1 from calibration Table	v	36·943	16·19	23·481	9·648
Volume v reduced to 0° and 760 millims..................	V_1	22·190	13·343	17·206	8·962
Volume in c. c. of b_1	v_1	71·218	50·875	57·968	45·254
Volume of solution (v_1-v).	h_1	34·275	34·685	34·487	35·606
Coefficient of absorption for temperature t and pressure P_1 millims. $\frac{\mathrm{V}-\mathrm{V}_1}{h_1}$...	α_1	0·7095	0·9562	0·8496	1·0545
Absorption-coefficient of distilled water for same temperature and pressure ...	β_1	0·6909	0·9631	0·8455	1·0718

TABLE III.

Determinations of the absorption-coefficient of a 1·23 per cent. solution of crystallized sulphate of magnesia for the temperatures and pressures indicated, the duration of the reaction being taken into account. V=47·650 c. c.

		1.	2.	3.	4.	5.
Barometer	δ	736·5	737·0	747·0	742·0	734·0
Thermometer	t	11·1	11·0	10·45	11·1	11·1
Outer level of mercury	a_1	63·0	257·0	446·5	506·0	553·5
Inner level in absorption-tube	b_1	174·5	230·5	265·6	277·0	303·0
Upper level of solution in absorption-tube	c_1	40·6	95·8	131·4	143·0	169·0
Upper level of water column in outer leg	f_1	3·0	195·0	382·0	440·0	488·0
Resultant water column $\{a_1-f_1-b_1+c_1\}$............	W	−73·9	−72·7	−69·7	−68·0	−68·5
Equivalent mercury column	q	− 5·46	− 5·37	− 5·15	− 5·02	− 5·05
Vapour-tension for $t°$.........	T	9·857	9·792	9·443	9·857	9·857
Resultant pressure on gas $\{\delta-\delta_1{}^*-\mathrm{T}+q\}$	P_1	8·327	695·3	551·5	498·1	468·6
Volume in c. c. of c_1 from calibration Table...........	v	9·905	22·106	30·07	32·67	38·52
Volume v reduced to 0° and 760 millims.................	V_1	10·429	19·441	21·016	20·576	22·824
Volume in c. c. of b_1	v_1	39·761	52·337	60·256	62·841	68·72
Volume of solution (v_1-v)...	h_1	29·856	30·231	30·186	30·171	30·200
Coefficient of absorption for temperature $t°$ and pressure P_1 millims. $\frac{\mathrm{V}-\mathrm{V}_1}{h_1}$...	a_1	1·2467	0·9331	0·8823	0·8974	0·8221
Absorption-coefficient of distilled water for same temperature and pressure......	β_1	1·3052	1·0445	0·8461	0·7456	0·7014

* $\delta_1=a_1-b_1$.

TABLE IV.

Determinations of the absorption-coefficient of a 0·205 per cent. solution of gypsum for carbonic acid at the temperatures and pressures indicated. V=48·759 c. c.

		1.	2.	3.	4.	5.	6.
Barometer	δ	737	738	738	743	743	743·5
Thermometer	t	10·1	12·9	13·3	11·1	11·1	11·65
Outer level of mercury	a_1	439·5	268	165	149	108	31
Inner level in absorption-tube	b_1	274·5	231·5	210	192·5	185·6	172·5
Upper level of solution in absorption-tube	c_1	153·5	111	85·5	65·5	57·6	43
Upper level of water in outer leg	f_1	426·0	238	126	101·5	54·5	−27·5
Resultant water column $\{a_1-f_1-b_1+c_1\}$...	W	−107·5	−90·5	−85·5	−79·5	−74·5	−71
Equivalent mercury-column	q	− 7·9	− 6·64	− 6·3	− 5·86	− 5·5	−52·4
Vapour-tension for $t°$	T	9·227	11·09	11·383	9·857	9·857	10·22
Resultant pressure on gas $\{\delta-\delta_1{}^*-T+q\}$...	P_1	554·87	683·77	765·3	770·8	805·2	869·5
Volume in c. c. of c_1 from calibration Table	v	35·02	25·5	19·83	15·42	13·67	10·43
Volume v reduced to 0° and 760 millims....	V_1	24·656	21·908	19·042	15·029	13·918	11·320
Volume in c. c. of b_1	v_1	62·27	52·56	47·73	43·80	42·25	39·31
Volume of solution (v_1-v)	h_1	27·25	27·06	27·90	28·38	28·58	28·88
Absorption-coefficient for temperature t and pressure P_1 . . $\frac{V-V_1}{h_1}$	a_1	0·8845	0·9923	1·0651	1·1885	1·2191	1·2964
Absorption-coefficient of distilled water for same temperature and pressure...	β_1	0·8617	0·9618	1·0624	1·1534	1·2048	1·2757

Comparison of the results of series I. and II. shows the effect which these sulphates have in altering the power of absorption of water for carbonic acid, when they are allowed sufficient time for the reaction. The subject of the absorption-power of saline solutions is one of much importance, and affords an almost inexhaustible field for research, when the effect of varying the nature of the salt, the strength of the solution, the temperature, the pressure, and the duration of the action of the solution on the gas are taken into account. I hope, at some future time, to be able to resume this interesting inquiry.

H.M.S. 'Challenger,' Simon's Bay, Nov. 4, 1873.

* $\delta_1=a_1-b_1$.

"On the Mechanism of Stromboli." By Robert Mallet, M.A., F.R.S. Received May 17, 1874*.

Stromboli and Masaya stand alone, so far as observation has yet gone, amongst the volcanic vents of our planet, in the remarkable characteristic of having a distinctly rhythmical intermittence and recurrence in their eruptive action. Masaya, though known for about 300 years, has been but little observed, so that some doubt may exist as to whether its action be truly intermittent and recurrent or not; and if we leave it aside for future observation, Stromboli stands unique amongst terrestrial volcanoes in the rhythmical character of its eruptions, more or less accurate observations as to which are upon record for above 2000 years. Every volcanic vent is indeed intermittent, and often recurrent, in its action, which has been properly denominated paroxysmal, but no law can be traced in the intervals of time elapsing between the paroxysms. A vent may suddenly open and a cone be thrown up, as in the case of Monte Nuovo, and after this burst volcanic effort may cease there, perhaps permanently; or, as in the case of Vesuvius, prior to A.D. 79, a period of repose may exist in a volcanic cone already formed, exceeding human local tradition, to be succeeded by paroxysmal efforts, varying enormously in intensity, and with intervals in time between successive eruptions varying from hours to centuries. In all these there is no rhythmical recurrence, or at least none that, upon the narrow scale open to our observation, can be viewed as such. In Stromboli, on the contrary, there is a distinctly rhythmical intermittence and recurrence, so regular in time, and preserving for centuries such a general uniformity in energy, and of such slight violence, as to point to some distinct train of mechanism as producing it—that mechanism, whatever be its nature, being comprehended within a moderate distance from the surface, and not referable to the more mighty and deep-seated forces which determine the uncertain and altogether unpredictable paroxysms of volcanoes generally. Not that the rhythmic intervals of Stromboli are precisely the same at all times, as has been erroneously stated by many travellers, nor the violence of its outbursts at all times alike; but both vary within narrow limits during the immense historic period that it has been observed. No satisfactory explanation has yet been given, so far as the author is aware, of the physical and mechanical condition constituting the mechanism upon which this extremely curious rhythmical action depends; and it is the object of this paper to point out what appears to be its real nature. It is the more worthy of attentive study, as Stromboli is in reality the link that connects two widely different phenomena—namely, the ordinary cone of eruption and the geyser. Stromboli is, in fact, a volcano and a geyser united and acting together in the same vent, the rhythmical action which characterizes the geyser

* Read June 18, 1874. See antè, p. 473.

being thus communicated, within certain limits, to the otherwise irregular and accidental activity of the volcano.

Passing ancient accounts, Stromboli has been visited in modern days, amongst men of science, by Spallanzani, Dollomieu, Hoffman, Scrope, Daubeny, and several others; but no very full or exact description of the crater and its adjuncts, much less any adequate explanation of the curious mechanism of its action, has been given by any of these writers.

Hoffman's account of the phenomena witnessed by him, though far from clear or satisfactory, is curious enough to deserve translation here:—"The volcano appeared to have changed into a hot mineral spring; then at irregular times we observed that the continually developing steam became stationary, and, with a jerking uncertain motion, rushed back into the mouth of the crater. At the same time we felt a terrifying trembling of the ground, accompanied by visible oscillations of the loose crater-sides. Then followed a hollow roar, and a volume of steam shot out of the crater, accompanied by a shrill crackling. Thousands of lava-fragments, which had been carried up with the steam, spread in the air like sheaves, and then fell back, either into the mouth or on the surrounding cinder and sand walls. We could distinctly see (particularly on this occasion) the boiling, seething lava dash against the sides of the shaft, separate into two streams, and then fall back; but the lava ejected in bubbles flew far through the air, twisting and tearing along, foaming drops, bright as cooled glass, clattering as they rolled down the declivity."

Mr. Scrope makes the following remarks in his 'Volcanos' (second edition, pp. 332–334):—

"The remarkable circumstance in this small but interesting volcano is that the column of lava within its chimney is shown, by the constant explosions that take place from its surface at intervals of from five to fifteen minutes, casting up fragments of scoriform lava, to remain permanently at the same height, level with the lip of the orifice at the bottom of the crater, and therefore some 2000 feet above the sea-level. It is evident from this that nearly a perfect equilibrium is preserved between the expansive force of the intumescent lava in and beneath the vent, and the repressive force, consisting in the weight of this lofty column of melted matter, together with that of the atmosphere above it; consequently a very small addition to or subtraction from the latter, such, for instance, as a change in the pressure of the atmosphere, must to *some* extent, however small, derange the equilibrium. It need not therefore surprise us that the inhabitants of the island, chiefly fishermen, who ply their perilous trade day and night, within sight of the volcano, declare that it serves them in lieu of a weather-glass, warning them by its increased activity of a lightening of the atmospheric pressure on the volcano—equivalent to a fall of the mercury—and by its sluggishness giving them assurance of the

reverse. It is the tension of heated steam or water disseminated through the lava in and beneath the vent which occasions its eruptive action, and the boiling-point of every drop or bubble must be sensibly affected by every barometric variation. . . . .

"In the foul weather of winter I was assured by the inhabitants that the eruptions are sometimes very violent, and that the whole flank of the mountain immediately below the crater is then occasionally rent by a fissure, which discharges lava into the sea, but must be very soon sealed up again, as the lava shortly afterwards finds its way once more to the summit, and boils up there as before. Captain Smyth found the sea in front of this talus unfathomable, which accounts for the remarkable fact that the constant eruptions of more than 2000 years have failed to fill up this deep-sea hollow."

Dr. Daubeny ('Volcanos,' second edition, p. 248) appears to have given but a cursory examination to the crater; and in his observations on its phenomena only repeats Spallanzani, Hoffman, and Mr. Scrope, as follows:—

"The unremittent character of the eruptions of Stromboli appears to arise, as Mr. Scrope has suggested, from the exact proportion maintained between the expansive and repressive force. The expansive arises from the generation of a certain amount of aqueous vapour and of elastic fluids; the repressive from the pressure of the atmosphere, and from the weight of the superincumbent volcanic products."

The mechanism, as imagined by Mr. Scrope, fails, in the author's opinion, to account either for the rhythmical character of the eruptions or for the alleged connexion between them and the state of the weather. No equilibrium between the "expansive" and the "repressive" forces can possibly exist at the moment of an outburst, the circumstances of which prove an excess of pressure of many atmospheres, which has been gradually increasing since the last outburst became quiescent.

To account for the actual facts, we must have such a train of natural mechanism as shall cause a gradual, though rapid, increase of steam-pressure within or beneath the vent or tube of the volcano, until the accumulated pressure suffices to overcome whatever obstacles it may encounter, solid or liquid, and by blowing these away release the pressure itself in a burst of steam, stones, dust, &c. The conditions producing this gradual increase of steam-pressure must be such as shall give rise to the rhythmical recurrence, at comparatively short intervals, of the phenomena. These conditions are certainly not to be found, either in the general nearness of balance of any expansive and repressive forces alone, or in any conceivable relation between these and variations of atmospheric pressure.

Mr. Scrope has, as the author believes, greatly overrated the altitude of the fundus of the crater in stating it at 2000 feet above the sea. But let us suppose the height of the column of liquid lava, between the level of the sea

and the fundus of the crater, to be one fourth of this, and the expansive and repressive agencies in the nicely balanced equilibrium assumed, what effect could any variation of barometric pressure, within the limits ever experienced on any part of our globe, produce in disturbing such equilibrium? A rise or fall of the barometer at the rate of a tenth of an inch per hour is known only to occur in connexion with the most violent hurricanes. A fall of half an inch in the mercury within three or four hours exceeds probably the utmost that occurs in connexion with the most violent Mediterranean storms. But let us suppose a fall of two inches in the barometer to take place instantaneously, how far would that affect the equilibrium supposed of such a column, however supported, and whether free from aëriform matter or containing vesicles thereof? Two inches of mercury are equivalent to about $\frac{1}{15}$ of the usual pressure of the atmosphere, or to less than one pound to the square inch at the sea-level. The liquid lava supposed to fill the column may be allowed to have a specific gravity of at least 2·000; a rise or fall, therefore, of a single foot in the top surface of this column would equilibrate this exaggerated amount and rapidity of barometric change. But the head of the column itself is described by Hoffman as continually in oscillation upwards and downwards through several feet. It is obvious, therefore, that changes of atmospheric pressure have nothing whatever to do with the mechanism producing the recurrent action of this volcano.

Whatever reality there may be in the notion, long handed down, of some connexion between the degree of activity of this volcano and changes of weather appears to be merely superficial, and the true interpretation will be referred to further on. In any case this notion of equilibrium within the chimney of this particular volcano, and its disturbance by changes of atmospheric pressure, would be equally applicable to every volcanic vent in the world, and fails to throw any light upon the special phenomena which characterize Stromboli, viz. the quasi regular recurrence of its bursts forth. The geysers of Iceland belch forth water and steam, and occasionally stones, and the order of recurrence is the same which characterizes those of Stromboli. The latter does not send forth water *en masse*, its ejecta being steam mixed with some gases, carrying up considerable masses of solidified lava, chiefly in angular blocks, mixed occasionally, but not always, with torn shreds and lumps of half-solidified lava, in a more or less plastic state, together with a preponderant volume of dusty pulverulent matter. It is highly probable that water, not in the state of steam, but in that of solid drops, is frequently blown from Stromboli, and such may be felt falling to leeward after some of the bursts forth, though not after all. It may be doubtful, however, whether or not these drops may arise from steam condensed in the air.

We thus have, to the same succession of phenomena as those of the geyser, superadded in Stromboli some of those of a volcanic vent, of feeble but long-continued activity.

The phenomena of geysers were for a long time supposed peculiar to Iceland; and although they are now known to exist elsewhere, their characteristics are nowhere better observable than in Iceland.

The recurrence of their outbursts, their duration and intervals, were very well described by Von Troil in his 'Letters on Iceland' in 1772, and have been further described by Sir George Mackenzie in 1810.

Henderson had ascertained that stones, or other obstacles, thrown into the geyser-tube influenced the interval between two outbursts generally by increasing it, and gave rise to augmented violence in the outburst when it came, the stones being projected back along with the water, and rising much higher than the latter, as might have been predicted from dynamic considerations. Sir John Herschel suggested an explanation of geyser-phenomena, based upon modifications of the mechanism long previously proposed to account for those of intermittent springs. His explanation, though tenable, certainly does not apply to all observed cases, and is scarcely likely to be the true one, because a much simpler mechanism has been since pointed out; and it may be taken as certain that, in explaining all natural phenomena, the simplest is the true one. This was discovered by Bunsen and Des Cloizeaux, who in 1846 examined the geysers of Iceland, and ascertained the fact that towards the bottom of the tube of the Great Geyser, at a depth of 78 feet from the lip of the basin, a thermometer immersed in the rising column of water rose to 266° Fahr., or to more than 50° above the boiling-point of water, under atmospheric pressure only; and these authors conclude that, as the flow of water which replenishes the tube after an outburst causes the aqueous column gradually to rise to the lip of the basin, the temperature of the water at the lowest part of the column continues to rise; and whether it receives its accession of heat from the sides of the tube or from jets of superheated steam issuing into it, no considerable volume of steam can be generated until the boiling-point has been reached at the bottom of the column, as due to its insistent pressure there, when a sudden and large outburst of steam takes place, and the whole column of water is belched forth from the tube, succeeded by the blowing-off of the pent-up steam which expelled it, and with steam evolved from the column of water as it rises, until that falls back to atmospheric pressure. The curious facts ascertained by Professor Donny, of Ghent, that water absolutely free from combined air may be heated to even 275° Fahr. before it boils, and then bursts into steam explosively, have been appealed to as auxiliary to the phenomena, but seem unnecessary, even were it certain that the water of geysers is absolutely air-free. Were it so, however, there can be little doubt that the rise in the boiling-point of such water, under atmospheric pressure, would also take place in the same water under a head of 78 feet, or equal to more than two atmospheres, and thus would still further augment the temperature at the bottom of the tube, and further increase the violence of the outburst.

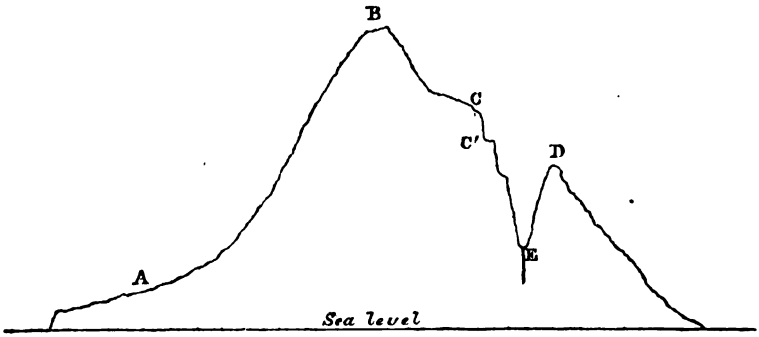
Bunsen is of opinion that the above is simply the mechanism of the Great Geyser; but that to account for some of the minor phenomena of the second, or Strokur Geyser, some additional mechanism, not widely differing from that suggested by Herschel, may be necessary. Some of the relations which subsist between geyser-phenomena, as thus explained, and those which he supposes to occur at various depths in the tubes of active volcanic vents, have been well discerned by Sir Charles Lyell, and are described in his 'Principles of Geology,' 10th edit. vol. ii. p. 220; but he has not applied them in explanation of the rhythmic recurrence of the outbursts of Stromboli. From Bunsen's explanation, as above sketched, it follows that the interval between two outbursts depends mainly on three conditions—the depth and capacity of the tube, the rate at which the water that fills it is supplied, and the rate at which heat, from whatever source, is transmitted to the water. Were these three all perfectly constant, the interval between two successive outbursts would be always the same, but it must vary, more or less, as any one of these three conditions may be altered. Again, the duration of the outburst, or time occupied in the expulsion of the column of water, and the height to which it is sent, as well as the volume of the jet, depend upon the capacity of the tube and the height to which the water rises within it before the blow-out commences, and must therefore vary in time with these conditions. The depth and capacity of the tube may vary secularly or be deranged suddenly; the temperature of the infiltrated water may vary, and therefore the time of its boiling under given conditions may alter with the season; and the temperature of the sides of the tube, and of the steam blown into it from fissures, must vary with the intensity of neighbouring volcanic action whence these are drawn.

Before proceeding to connect the circumstances presented by Stromboli with the above facts in relation to geysers, it will be necessary to adduce some facts in relation to the former, derived from personal observation.

In the latter part of the year 1864 I examined the whole of the Lipari Islands, with the exception of Felicudi and Alicuda, which the lateness of the season rendered impossible. Starting from Cape Mellazo (Sicily) in a "well-found" speronala, with a crew of eight men, which I retained throughout the voyage amongst the group of islands, I had the pleasure and advantage of being accompanied for some days, and as far as Lipari Island, Panaria, and Stromboli, by my friend Colonel H. Yule, B.E., well known for his embassy to Siam, and recently for his noble edition of Marco Polo's travels, and by various other works. Our landing at Stromboli was difficult, from the high surf running in; and after our arrival the weather became so much more tempestuous as to detain us there some time. We enjoyed the hospitality of Padre Capellano Giusseppe Ranza, whose house is situated not far from the central parts of the island, and whence a steep but not difficult walk leads up to the

crater and to the highest point of the island. The statements which have been made as to the relative heights of different points of this island appear to be only derived from guess, and are greatly in error, as I am enabled to show, although I am not in a position to give heights which are rigidly correct, my hypsometric measurements having been made by means of a single aneroid.

Diagram No. 1.



The *pergola* of Padre Ranza's house (marked A, Diagram No. 1) was found to be 211 feet above the sea at St. Vincenzo, and the highest point of the island (marked B) is 2843 feet thus measured. Captain Smyth, however, gives the height as only 2576 feet: this was probably taken by him by the usual nautical methods of triangulation, and if so, may not be more exact than my own rough barometric measurement. The height of the ridge overhanging the crater, marked C, was in like manner found to be 1200 feet. We were enabled to clamber down from this over crags of lava, whose irregular terraces and ledges were capped, more or less deeply, with black volcanic sand, containing immense quantities of crystals of augite, down to a point overhanging the landward wall of the crater, and at no great distance from its verge, from whence we witnessed the phenomena of eruption. This point, marked C′, I found to be 904 feet above the level of the sea. From this point the great, irregular, and somewhat oval funnel-shaped crater was before us; and looking seaward the highly irregular walls bounding its edges sloped towards the sea, and were united transversely by the sharp irregular edge or summit of the mass of broken and in great part wholly discontinuous and angular ejected fragments, which form a slope down to the sea, between the opposite sides or jaws of the cove or reentrant angle in the coast-line called the Schiarrazza. From the point where we stood this edge (D on Diagram) was estimated by the eye and clinometer at about 300 feet below us; and the narrower width of the crater thus seen across at its brim I estimated at from 300 to 400 feet. The form of the crater as described by Smyth ('Sicily and its Islands,' p. 255) in 1824 was stated to be circular, and

its diameter about 510 feet. This statement can only be received as approximate, as at that date the brim of the crater cannot have been extremely different from what it was in 1864; and the bounding walls, which are of material the greater part of which is as ancient as is the island itself, can scarcely admit of its ever having been circular, or much different from the irregular gulf it presented when I saw it. From our position of observation, every thing around us was of the sable colouring of black lava and volcanic sand. We could not see with any distinctness the fundus or bottom of the crater, a cloud of vapour issuing from its bottom, and in places from its sides, nearly filling the cavity, and obscuring the bottom even between the outbursts. This vapour smelled strongly of hydrosulphuric-acid gas. At all the lower part, as well as I could discern, the steep and solid walls of the crater merged into a very steep funnel-shaped talus of loose materials, at the centre and bottom of which was the aperture of the tube or "chimney" of the volcano. This has been described by Hoffman as entering the funnel by three apertures. Judging from the form of the column of steam, dust, stones, &c., as seen at the first moment of ejection, the aperture appeared to me to be a single one, irregular in form, and with its longest dimension in the direction of the greatest width of the crater itself. Looking down from our position over the foreshortened slope of black débris which plunged into the sea 900 feet below us, the two jaws of the Schiarrazza are seen to be composed of huge broken-off beds of lava, which dip to seaward at various depths below the surface; these, partly by superficial decomposition, partly by being covered with serpulæ and corallines, are of a nearly white colour; and as we stood with the sun at our backs, the sea above these beds, at either side of the Schiarrazza, on which the sun was shining, presented the most glorious tints, varying with the depth of the water from golden-yellow to the purest emerald-green, while between these, and looking right down over the black slope of débris, the deeper sea was of an intense indigo, passing off into blackness. Nothing in the way of natural colouring and wild outline combined could exceed the weird horror and intense beauty of contrast when a burst from the volcano sent forth in the midst its volumes of white steam and dust, which, seen by the reflected light of the sunbeams shining through it, appeared of every tint of ruddy brown or blood-red. From what precedes, and by reference to Diagram No. 1, it will be seen that the bottom of the crater-funnel cannot be more than 300, or at most 400 feet above the level of the sea where the tube or tubes enter it, and that the statement made by Mr. Scrope ('Volcanos,' p. 332, 2nd edition), viz. "the lip of the orifice at the bottom of the crater is some 2000 feet above the level of the sea," is largely in excess of the truth. Were that a fact, the brim of the crater, which is 300 to 400 feet above the bottom, would be situated within a height of about 175 feet according to Smyth's measurement, or within about 300 to 400 feet according to my measurement of the highest point of the

island, either of which is physically impossible. While we remained observing, the outbursts from the bottom of the crater were found to be very irregular as to time, varying, as timed by the watch, from a minimum of two minutes interval to a maximum of thirty minutes, and in one case, after we had commenced our descent, to forty minutes. I could not trace a very distinct correspondence between the largeness of this interval and the violence and volume of the outbursts following it, although the tendency seemed to be to such a correspondence; and the duration of the outburst was certainly greater as the interval between two was so. At each outburst a huge volume of dust and small material, and with more or less of large fragments of solidified lava, all angular or subangular, and with a few fragments and shreds of different sizes of lava still hot enough to be slightly plastic while falling, were ejected; none of the fragments were of any great size, none appearing to exceed in weight about 500 or 600 pounds, and none of the pieces of plastic lava reaching half this weight. The light wind blew from us towards the sea, out over which a portion of the finer dust was wafted after each outburst; but the great mass of the dust and fragments, whether small or large, fell back into the crater upon its bottom and steeply sloping funnel, a few only, and generally of the largest fragments, being thrown out over the crest of the crater at its sea side, and landing amidst the débris of the slope, down which they clattered. It was obvious that the orifice of the tube at the bottom of the crater was greatly obstructed by the loose material forming the funnel above it, which seemed after each outburst to be continually slipping, more or less *en masse*, and so blocking up the tube, along with the mass of ejected material which dropped back upon the orifice; for it was easily remarked that successive outbursts apparently took place from different points, distant occasionally some yards from each other, in the bottom of the crater—the main axis of the column, or its greatest thickness, varying thus in position, and also more or less diverging slightly from the vertical, sometimes one way and sometimes another, as though the ajutage of discharge was through loose material of partly large and entangled blocks, mixed with finer material, the positions of which were more or less altered after each discharge. None of the large fragments which we saw thrown out rose higher than the position at which we stood, and few even so high—that is, they did not rise more than 400 to 500 feet above the orifice at the bottom of the crater; but occasionally the height of projection must a good deal exceed this, as I found many angular fragments and large shreds of lava, which had fallen in a leathery or plastic state, to the landward and eastward sides of the brim of the crater, 150 feet or more above the level of our point of observation. The black sand and dust and crystals of augite are found in large masses still higher and further from the crater on the land side; but much of the latter are blown inland by the strong winds from the northward that prevail in winter. The solid mural precipices which form

the walls of the crater above the funnel of loose material consist of beds of solid lavas and agglomerated fragments, and appear to dip more or less towards the sea, or away from the centre of the island, and were no doubt formed by one of its great early and more central craters at a period excessively remote. *Suffioni* of steam issue in some spots from between these beds, and the percolation of water was seen in places not far below the brim of the crater. There is a perennial spring of percolated water much higher up upon the island, and under the steeps that mark the position of an ancient crater, so that it is highly probable that rain-water, in greater or less quantity, finds its way into the funnel, and even the tube, of the volcano, although the percolation of sea-water is no doubt the chief source of the supply, which is blown out as steam, and perhaps in part as pulverized water. Each outburst, while we continued to observe them, was preceded by several distinct low detonations, with intervals between each of from 4 or 5 seconds to as much as 80 seconds: these, though of a far deeper tone, greatly resembled the cracking noises that are heard when steam is blown into the water of a locomotive tender for the purpose of heating it. These detonations sensibly shook the rock beneath our feet.

The outburst, when it comes, does not rush forth quite instantaneously or like that of exploded gunpowder. It begins with a hollow growl and clattering sound of breaking or knocking together of fragments of hard material, which very rapidly increases to a roar at its maximum, continues at about the same tension for a period varying from a few seconds to a minute or two, and then rapidly declines, but less rapidly than the augmentation took place. At the first instant of the outburst, the rock on which we stood was very sensibly shaken, the vibrations being both vertical and more or less horizontal; at the end, and after the fragments have ceased to fall and the dust has cleared away, all tension of vapour in the tube seems for the moment at an end, and the funnel is seen filled merely with rolling clouds of vapour. The noise produced by the outburst is not very loud, and more resembles that of the rush of a heavy railway-train over an iron-girder bridge, when heard at some distance off, than any other sound to which I can compare it, but more fluffy and flat. On examining the existing surface of the island, it is easily discerned, by an eye educated to the observation of extinct volcanic regions, that successive craters have been formed, shifting their positions posterior to the production of that great and nearly central one from which the main mass of the island was thrown up. The existence of three such craters may be traced; and the existing little crater is situated at the landward or south-eastern side of a vastly larger crater, all the north-western or sea side of which has been destroyed and buried in deep water, and of which the heavy beds of lava seen under water at both sides of the Schiarrazza are the only remains to seawards of the existing slope of débris. This is represented by the Diagram No. 2, copied from my note-book, in which

the diagonally shaded portion represents an ideal section of the island as it now stands, taken through its highest point and the existing crater of Stromboli, while the lines beyond indicate the probable outlines of the island when the great crater was active, of which the Stromboli of to-day may be viewed as little more than a fumarole.

Diagram No. 2.

I may add here, in reference to the Lipari group generally, that all the islands present more or less distinctly these characteristics of craters whose axes have shifted and formed new ones at immensely ancient epochs, and with vast intervals of time intervening.

The entire group presents, though in various degrees in the different islands, the general character of great decadence of a once far more powerful volcanic activity; and, in every case, as the cone, or rather mound, of each island increased in mass and height, the original vent thus increasingly obstructed tended to move off and open new and easier vents in directions approaching the coast-lines, just as in the case of very old and massive volcanoes on land, such as Etna, migrations have occurred of their most ancient great craters, and, in more recent times, new ones have opened low down upon their flanks, such as Monte Rosso &c. The epoch of primordial activity was far from contemporaneous in all the islands; and we find in them now the most varied stages of volcanic decadence. In the island of Vulcano, we have an empty crater of enormous capacities and depth (1100 to 1200 feet), the bottom of which reaches to within a few feet of the sea-level, and is only separated from the shore-line at the north-east of the island by an extremely steep bank of compact tufa. The oldest craters having been situated much more centrally and far to the south-west, while the little crater of Vulcanello was thrown up to seaward of the ancient coast-line and between it and the deep crater just spoken of, boiling springs and boiling streams of superheated vapours issue below the sea-water, and a thermometer sunk amongst the pebbles of the beach in many spots rises to above 300° Fahr. In the bottom of the deep crater the principal " bocca," which is several feet in diameter, and though only blowing out superheated steam and gases with a mea-

sured sort of rise and fall in its snorting, is red-hot to the lips, which are of hard lava; and the temperature at the mouth I found, in 1864, was sufficient to melt brass wire, but not sufficient to fuse a similar wire of bronze (*i. e.* copper with about 5 per cent. of tin).

The falling-in of any considerable proportion of the walls of this grand crater, which, on the landward side, consist of nearly horizontal beds of volcanic rock and conglomerate, forming at that side a vast mural precipice, though almost wholly of compacted tufa for the remainder of its periphery, would easily give rise to a renewal of volcanic energy, such as nowhere exists now in the Lipari group.

Stromboli is the next to this in existing energy; but though the persistence and character of its activity excite much more attention, the actual evolution of volcanic heat is greatly inferior to that constantly going on from the unobstructed "bocca" of Vulcano, which, if again obstructed, would produce very violent effects. In Lipari Island we have the traces of several great craters of extreme antiquity, the most recent being that which evolved the mountainous masses of pumice and the enormous stream of pumice and obsidian which falls into the sea at the north-east of the island; but the greatest sign of present activity is found on the shore at the opposite side of the island, at Il Stufi, where innumerable jets of hot water and superheated steam with sulphurous acid issue from the heavy beds of trachyte, which they rapidly decompose and convert into clays and hyalite.

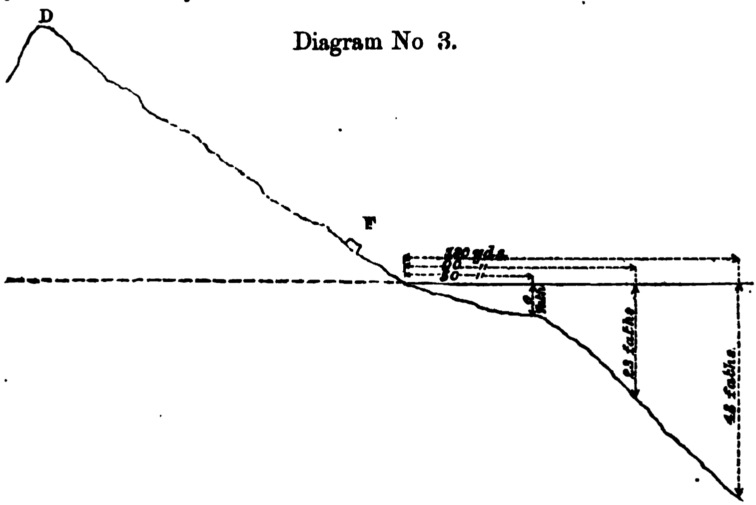
In others of the islands, such as Panaria and Saline, no sign of activity remains, and the most practised eye with difficulty seeks to recover the positions or outlines of the very ancient craters. Lastly, in the small islands of Basiluzzo, in the low rocks of Liscanera and Liscabianca, and in the huge spire of Datola, formed of vertically parted and splintery trachyte of the most obdurate character, we have but the last shreds of one or more great volcanic islands which once occupied the shallow sea-spaces between all these islands, and probably connected them into a single vast cone. A hot spring still rises in water of 4 or 5 fathoms deep between Liscanera and Liscabianca, which possibly mark the site of one of the most recent of the craters at this spot, the islands which they formed, at a period too distant for imagination, having here almost disappeared under the eroding influence of the comparatively tranquil and tideless waters of the Mediterranean, aided perhaps by local subsidence, but of which there is little or no evidence. It will thus be seen that the change in position and decadence in energy ascribed to the existing crater of Stromboli, although for 2000 years its energy has seemed to be constant or not greatly diminished, are circumstances in complete accordance with the facts presented generally by the volcanic islands of the whole group. The existing tube of Stromboli, like that which leads to the "bocca" of Vulcano, has but a lateral and indirect and very much choked-up communication with those great central ducts which once gave vent to

the products of the great craters. Lava from these, generally imperfectly melted, but occasionally in a complete state of fusion, still finds its way from these into the upper parts of the existing tube of Stromboli, but in comparatively very small quantity.

On leaving St. Vincenzo we circumnavigated the island of Stromboli, and examined the slope of débris in Schiarrazza cove; the actual average angle of slope is much overrated by Mr. Scrope at 50° ('Volcanos,' page 32). By the clinometer it proved to be from 34° to 36° with the horizontal. The slope consists almost wholly of angular fragments, averaging but a few hundredweight each, and of shreds and tails of lava that have fallen in a semifused condition. Mixed with these, in a wholly irregular manner, are here and there sinuous and twisted flakes of lava. These have been often taken for dykes of lava forced out by hydrostatic pressure through the bank of débris, when the crater has been assumed brim-full of liquid lava; but I am not aware of any evidence whatever in support of the notion that this crater ever has been so filled. Its steep walls present no traces of the contact of liquid lava at any time since their formation; and it can scarcely be doubted that had the crater ever been filled with liquid lava to the brim, the loose and incoherent slope of débris would have been utterly unable to sustain the pressure, and must have been forced bodily into the sea, into which the mass of liquid lava must have followed it. The base of the slope appears to consist of solid and, no doubt, comparatively water-tight beds of lava, like those described, as seen from above, at both sides of the Schiarrazza; and but for these the existing crater could have never been formed, or its activity preserved, for it must have been drowned out by the inroad of the sea, as so many other and recent craters in the other islands prove to have been. The sinuous masses of lava seen at various parts of the slope of débris appeared to me no more than huge splashes of very liquid lava, which, in outbursts of greatly more than usual intensity (such as was one of those witnessed by Hoffman) and with a larger supply of lava than usual, were blown out over the crest of the slope and fell amongst the blocks of débris. Fresh deposits of débris obscure the features of most of these plashes; but I observed, in some cases, that the lava had distinctly moulded itself, like a mantle, to the sinuosities between and the forms of the blocks upon which it fell. Within a yard or two of the base, or water-line, of the slope were two blocks of lava of exceptional magnitude, the larger having a volume of 8 or 10 cubic yards. These blocks were confidently affirmed to us to have been projected during some violent burst forth and thrown clean over the crest of the slope, and to be in fact *blocs rejetés* thrown from the bottom of the crater; examination proved that they could be nothing of the sort. They were sharply angular, and all the surfaces had the crystalline texture of dark pyroxenic lava not very long fractured, except in some places, where distinct signs of weathering were evident in the larger block. Had they been *blocs rejetés*

they would have presented on all their surfaces and edges the more or less rounded outlines and extreme induration and closeness of grain due to long-continued torrifaction, which are the invariable characteristics of such blocks. The true history of these great blocks is, that they had been detached by the shakings of the outbursts from one of the steep cliffs of the ancient crater-walls which overhang the crests of the slope, and had thence rolled down to the position in which we found them at about F in Diagram No. 3, which is a section of the slope of débris and of the sea-bottom in line extending from its base. In this line we took a few soundings at distances from the water-line at the base of the slope, which we had to guess. These distances, as guessed by me, exceeded those guessed by Colonel Yule, though not very greatly; and I have preferred to adopt those derived from his military experience in guessing distances by the eye rather than my own.

Diagram No 3.



It will be seen from these soundings that the statements made by the islanders, and wrongly attributed to Captain Smyth (see his 'Sicily' *in loco*), that the sea outside Schiarrazza cove is unfathomable, and hence swallows up the débris of more than 2000 years, is wholly erroneous. Indeed Smyth's soundings ('Sicily and its Islands'), as well as the Admiralty Chart, sufficiently indicate that for some miles in the offing here the Mediterranean does not exceed 100 fathoms in depth. The bottom along which I took these soundings consists of huge irregular and ovoidal masses, of 10 to 20 tons in weight, of volcanic rock, old and water-rounded. What, then, does become of the débris? Its quantity, in reality, is extremely small in a given time. A very large proportion of it consists of mere dust and glassy or angular lapillæ, and these, if blown seaward, fall at a considerable distance away, and are lost in the depths; those which fall nearer, including the fragments that form the average mass of

the slope, are slowly removed and carried out into deeper water by the under tow of the heavy seas in winter, and are lost in the chinks and crevices between the huge blocks at the bottom, which I found to be so deep and tortuous as often to render the extraction of the sounding-lead which had entered them difficult. The lava ejected by Stromboli, whether in the solidified or half-melted state, is extremely dark-coloured, almost black. It is highly pyroxenic and crystalline, and its fusibility is greatly increased by an intimate intermixture of dark obsidianic glass, of which particles as well as strings are met with everywhere. It is still leathery or plastic at a temperature considerably below a red heat, visible in daylight, and is probably in tolerably liquid fusion at a temperature not much exceeding 1200° Fahr. Its composition is by no means invariable, as may be seen on the slope of débris; and when the glassy material is very abundant, as from time to time seems to be the case, or from any of the causes which influence periodically the fluctuations of temperature more or less observable at all volcanic vents, this lava would become extremely liquid and be blown about by the outbursts in the way somewhat obscurely described by Hoffman, as well as urged in liquid flakes over the brim of the crater. The crystals of augite which are deposited in such abundance with the dust blown out may preexist in the lava, but appear to me, more probably, to be mainly formed by the disintegration of the hot lava by contact and churning up with water, under a considerable pressure and therefore high boiling-point, and perhaps by separation and recombination from solution of its constituents. The crystals, which are often an inch or more in length and frequently macled or cuneiform, have scarcely any lustre; and when the surfaces are closely examined with a lens, they are often seen to be minutely pitted with microscopic cavities. We now come to collect and correlate our facts and draw such conclusions as they warrant in explanation of the mechanism which produces the phenomena of Stromboli. The supply of water producing the immense volumes of steam constantly blown off at the rate, on the average, of three or four outbursts per hour may be derived in part from percolated fresh water; but this source alone, derived from the small gathering area of the island, would be wholly insufficient; and, were that the sole source, it would almost wholly fail towards the end of the dry season, so that a marked annual change in the volcanic phenomena must result, and could not fail to be observed. The supply of water, however, is manifestly regular, and very nearly constant at all times, and therefore is derived from the sea, and thus must enter the tube of the volcano below the sea-level—that is to say, more than 400 feet below the lip of the tube at the bottom of the crater-funnel. Whatever be the source of supply of the lava, therefore, it can never fill the tube as a solid column of melted matter reaching up to its lip; for in that case, whatever be the mechanism of the volcano at each outburst, the whole of this immense column of melted matter of more than 400 feet in height must be blown completely

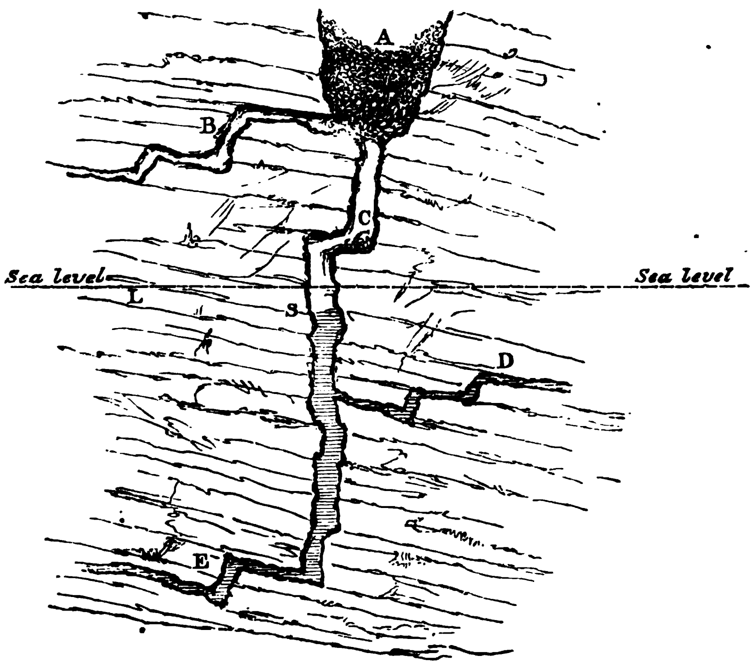
out of the tube, which actually is emptied, at the end of each outburst, of everything but gases and vapours, and these at a tension not greatly exceeding that of the atmosphere. We do not know the average section of the tube, and therefore cannot calculate the volume of lava that would be propelled thus out of the tube, if previously filled by each outburst; but it is manifestly so great that it would wholly change the character of the phenomena exhibited by the volcano, and must, during the last 2000 years, have produced a mass of ejected matter of enormous magnitude instead of the insignificant amount of mixed lava and débris which alone are to be seen.

Liquid or semiliquid lava does, however, continually make its way into the bottom of the crater-funnel and amongst the fragments collected there, which it more or less solders together, and along with which it is blown out at the outburst. Some may ooze into the tube lower down, and may more or less obstruct, but can never completely fill it. The walls of the tube, and those of all the fissures or cavities below the level at which the more or less fused lava reaches them, can scarcely have a lower temperature, and are most probably at a higher one than the lava itself. If the tube of the volcano were the main, or only, ajutage through which the liquid lava, as well as the steam to blow it out, were supplied—if, in fact, the tube were the main duct into the lowest depths of which both the liquid and vaporous matters entered—then, at irregular intervals, the tube, and even the crater, must become filled, and the whole phenomena of eruption would not differ in character from the highly irregular paroxysmal efforts of any common volcano of like energy. The tube, then, here plays a different part from what it usually does, and constitutes an additional element in its machinery, upon whose action in producing expulsion the rhythmical recurrence of the outburst depends.

We can now discern the very simple mechanism by which the actual phenomena are produced, a description of which will be rendered more intelligible by reference to the ideal Diagram No. 4, in which A is the lower part of the funnel of the crater, filled more or less with the fragmentary mass which has fallen back into it from the preceding outbursts. B is a lateral duct conveying more or less liquid lava into the bottom of the crater. C is the tube leading to the bottom of the funnel from a depth considerably below the sea-level, supposed to be, at the line L, at about 400 feet below the upper lip of the tube. D is a duct communicating with the sea, and enabling sea-water to find access to the interior of the tube, and to rise therein, if otherwise unimpeded, to the sea-level. E is either a lateral duct or a continuation of the tube itself, through which steam at a high temperature and tension enters the tube at some point much below the level of the sea. The lava- and steam-ducts, B and E, may be supposed to come from the ancient great volcanic channels still remaining under the more central parts of the island, and which supplied its great ancient craters. The duct D may consist of many

small fissures, permitting sea-water to percolate through the mass of consolidated and hardened volcanic rock, which, at this depth, constitutes the mass of the island, and in which all three classes of ducts or channels, B, D, and E, are formed, most probably between the partings of superimposed beds; or a single large duct may bring in the supply of sea-water, and have such a form, however irregular, as to preclude the steam in the tube from blowing out into the sea, although having a tension many times greater than the direct statical head of the sea-water itself *.

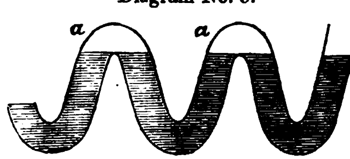
Diagram No. 4.



All of the three ducts, B, C, and D, may be multiform, irregular, and varied to almost any extent, provided only that they retain their relative positions, and these only within certain large limits, as shown in the Diagram No. 4. Supposing an outburst finished, and its fragmentary matter partly returned to the bottom of the funnel of the crater at A, the required conditions of whose form are only such that the fallen-back

* The well-known phenomena of a tube repeatedly bent in a vertical plane filled with water, but containing air or vapour at the upper part of each of its bends, as at *a*, *a*, Diagram 5, described in almost all large treatises on hydraulics, may be referred to as sufficiently illustrating what has been above stated.

Diagram No. 5.



débris shall have some sort of landing-place and support for the larger portion of the mass, so as not all to drop into and permanently block the tube. The lava oozing from the duct, or ducts, B, escaping amongst these fragments, solders them more or less together; and in proportion as its rate of supply is greater or less, some of it may overflow and drop, in a more or less liquid state, into the tube C and into whatever water it may contain. The tube, however, is emptied at the outburst of nearly all that it contains, and the tension therein being that of the atmosphere, or little more, the sea-water again begins to fill it by the ducts D. This water is already probably considerably warmed in the ducts D; it receives accessions of warmth from the sides of the tube and from the continual blowing into it of superheated steam and vapours through the ducts E, whose temperature is probably not far different from that of the lava at B. The column of sea-water rises in the tube to a level, we will suppose S, by which time the boiling-point has been reached at the lowest point of the column, namely, that due to the statical head of water, and to all such obstructions above the lip of the tube as tend to hinder the escape and so increase the tension of the vapours and gases occupying the otherwise empty upper part of the tube. At such an instant, the whole column may be lifted through a few inches or feet vertically by the steam locally generated at the bottom of the tube; and as this incipient evolution of steam escapes upwards the whole column of liquid will be suddenly dropped back upon the bottom of the tube, to be again similarly lifted, and so on until every portion of the column of water has acquired the full boiling-point due to its depth, &c. This is the cause of the detonations heard before the outburst. As soon as this has been reached, the whole mass of water below S is driven violently upwards, and partly by its impulse, but mainly by actual steam-tension, drives before it the mass of obstructing matter filling the bottom of the funnel at A, and the whole is driven forth together in a mingled cloud of dust, stones, shreds of half-melted lava, steam, and pulverized water. When the tube is left empty, and after the fall back of the fragments, the whole apparatus is ready for a repetition of the process. It is obvious that the depth of the tube below the level of the sea, and the temperature of its sides and that of the steam entering at E, determine the force of the outburst, that the rate of supply of water and steam determine the intervals between the outbursts, and that the proportion between the volume of steam and that of pulverized water, at each outburst, depends upon the capacity (that is, the greater or less section) of the tube C. If that be narrow in proportion to its total depth, as is probably the fact, then very little water will be ejected in any state but that of steam. It is not necessary that the temperature of the column of water in any part of the tube C should ever reach the tension due to a temperature equal to that of the lava escaping from B; it only needs to be such as shall raise its own column to the lip of the tube and overcome the obstructions there encountered with a sufficient residual tension left to blow these a greater or less height

into the air. The augite crystals are probably formed within the tube, from small portions of lava dropping in a liquid state into the water it contains.

Reverting now to the remarks made at the beginning upon the relations traditionally said to exist between the phenomena of this volcano and the state of the weather. It is obvious that the notions of nicely balanced equilibrium in a tube always filled to the lip with liquid lava can no more account for any such relation with the weather than it can explain the rhythmical recurrence of the outbursts themselves; and if supposed relations with changes of weather, as alleged to be indicated by Stromboli, could be thus explained, every constantly active volcano in the world would be equally a "weather-glass." Kilauia, for example, must present upon an exaggerated scale all the weather-prognostics attributed to Stromboli. In examining the vague statements made upon this subject, we should bear in mind the extreme incapacity of ignorant peoples to observe phenomena with accuracy, their proneness to exaggeration, and the readiness with which they accept traditional statements, however improbable. The statements made to me by several of the more intelligent people of Stromboli as to the height to which stones were alleged to be thrown, viz. far above the highest point of the island, as to the filling of the crater brim-full with liquid lava (which, however, no one had actually himself seen), and the forcing through the slope of débris of vertical dykes thereof, as well as the projection of the huge blocks we saw at the bottom of the slope, and such like, should be borne in mind before we attempt to square theoretic views with statements of facts that probably have no real existence. The only intelligible statements that I could gather from the inhabitants of Stromboli as to relations between the weather and their volcano resolved themselves really into two propositions: first, that in fine weather the light reflected upwards from the crater was more brilliant, and apparent at a greater distance, than in windy or uncertain weather; secondly, that in cold and broken weather the light was diminished, and a heavy cloud of vapour hung more or less over the crater.

These are intelligible facts, and admit easily of being accounted for on well-known meteorological principles. A tendency, though not a marked one, to the production of sea- and land-breezes in the morning and evening is observable in these islands, the sun-heat during the day being often very great, as also the nocturnal radiation. These, taken in connexion with the prevailing direction of the wind at a given time, viz. whether it sweeps over the island and over the highest points from the southward and eastward, or blows against its steep north-western face and into the crater, will, by altering the state of the atmosphere above the latter, tend to produce changes both in the light and in the vapour-cloud of the volcano. But that there is any real connexion, in the way of direct cause and effect, between the energy or frequency of the outbursts and the state of the weather, or fluctuations of barometric pressure, or *vice versâ*, seems devoid of any foundation whatever.

"A Contribution to the Anatomy of Connective Tissue, Nerve, and Muscle, with special reference to their connexion with the Lymphatic System." By G. THIN, M.D. Communicated by Professor HUXLEY, Sec. R.S. Received April 22, 1874*.

I published in the 'Lancet' of the 14th February of this year a paper entitled "On the Lymphatic System of the Cornea," in which I endeavoured to show that the canals in that structure in which the nerves lie communicate with the lacunæ, that the straight canals and lacunæ are connected by means of a continuous layer of flat cells, the margins of which are indicated by the well-known action of nitrate of silver, and that these cells are not the anastomosing so-called cornea-corpuscles, but that the flat cells line the lacuna, while the branched cells fill the cavity.

I have lately undertaken a series of further investigations on the same subject.

In order to corroborate the results yielded me by the nitrate of silver, I availed myself of the well-known property which hæmatoxylin possesses of specially staining the nuclei of cells. I allow the cornea to remain in the solution until it is perfectly saturated. Subsequent maceration in acetic acid removes the hæmatoxylin from the fibrillary substance before it bleaches the nuclei. On comparing a cornea so treated with successful preparations of the cornea-corpuscles as obtained by chloride of gold, it is found that the number of cells demonstrated by the hæmatoxylin exceeds by several times that found in the gold preparation, affording direct proof of the existence of other cells in the cornea than those known as the cornea-corpuscles.

If a vertical section of the cornea is so treated by hæmatoxylin and acetic acid, in many of the clefts in the fibrillary substance, in which, as is well known, the cornea-corpuscles are situated, several nuclei are seen, proving in another way the existence of a greater number of cells than those hitherto accepted by anatomists.

But in addition to the proof afforded by staining the nuclei of the cells, I have, by the application of a new method, been able to isolate (and thus demonstrate beyond all further possibility of doubt their existence in the cornea) a large number of cellular elements, the varied size and shape of which distinguish them not only from the cornea-corpuscles, but from any anatomical structures that have been as yet described.

If a cornea is placed in a saturated solution of caustic potash, at a temperature between 105° and 115° Fahrenheit, it is reduced, in a few minutes, to a white granulated mass of about a fourth of its previous bulk. In a small piece of the diminished cornea, broken down with a needle and examined under the microscope in the same fluid, it is found that the only visible elements are a great number of cells. If the con-

* Read June 18, 1874. See antè, p. 380.

junctival epithelium of the cornea has not been previously removed, the cells of that structure can be recognized amongst the others; and if the mass under examination has not been too much broken up in manipulating, groups of them may be seen in direct anatomical continuity with long narrow flat cells, which belong to the elements that have been for the first time brought to light by the potash solution.

But the cells of the anterior or surface-epithelium form a very small proportion of the number. The smallest piece that can be removed by the needle from a cornea which, before being put into the solution, has had this epithelium scraped off and Descemet's membrane removed, shows under the microscope a multitude of cells. Of the branched corpuscles, the fibrillary substance, and nerves, not a trace is visible.

The form of these cells is so various that it would be difficult to construct a series of types under which every individual cell could be brought. They seem in their development to have assumed any modification of form that is necessary to enable them to fit accurately the cavities and fibrillary bundles to which they are applied.

Those whose outlines do not permit their being accurately described as belonging to a strictly defined type, are many of them somewhat quadrangular or triangular in form, or club-shaped, with a short or long projecting process. Of fixed and definite types are long narrow rods, ending obliquely at the point, and oblong cells intersected at one end by a notch, which receives the extremities of two of the long cells that lie parallel to each other.

I do not attempt to give an exhaustive account of the various forms assumed by these cells. A better idea than can be given by any description will be got by an examination of figs. 1, 2, 3, Plate VIII., in which many of them are represented; but an examination of the first prepared cornea will show that there are many forms and modifications which have not been drawn.

The cells are granular in appearance, with sharp clear outlines. The terminal surfaces of the long cells can often be seen to be finely serrated; and so closely do they fit each other at these points, that sometimes a high magnifying-power is necessary to discover the suture-like line by which the junction is indicated.

The nuclei of all the cells have nearly the same length, but in the narrower cells the nucleus is often much compressed transversely.

The long cells are many of them 0·09 millim. long and from 0·006–0·003 millim. broad; the shorter cells are broader. Those 0·06 millim. long are generally about 0·009 millim. broad. A length of 036 millim., with a breadth of about 0·015 millim. is common; others are 0·03 millim. long and 0·012 millim. broad.

I have chiefly examined the cells in the cornea of the ox, sheep, and frog, and have found no important differences either in shape or arrangement.

In examining portions of the cornea which have been as little dis-

turbed as is consistent with the maintenance of transparency, groups of cells are found massed together *in situ*, as they have been left by the dissolving out of the fibrillary substance by the potash: these are found chiefly in two forms. Transverse masses of the anterior epithelium are found sutured to long narrow cells, which sometimes seem to join them at an angle.

Further, flat quadrangular masses of a single layer of cells are found formed in the following manner:—Of two opposite sides the external rows are formed of more or less rounded and angular cells, to which are joined long narrow cells that lie parallel to each other. Those from each side respectively meet in the centre, where they join. The remaining sides of the quadrangle are formed by a side view of these various cells, where they have been detached from the adjoining ones in the breaking down of the cornea mass.

The coincidence between the breadth of the long narrow cells and the fibrillary bundles of the cornea-substance, as seen when prepared by the ordinary methods, is evident, the continuous planes formed by their junction indicating that they form layers between which it is enclosed. According to this view, the ground-substance is everywhere encased in a sheath of cellular elements.

Bowman's corneal tubes I believe to include both the straight canals described in the paper above referred to and the spaces between the long cells widened by injection, chiefly the latter.

Although I have nothing to add to the description of the mode of preparation which I have already given, I must state that there are conditions of success, as to the nature of which I have not yet come to a definite conclusion. Sometimes the same solution, applied at the same temperature to different corneæ, succeeds in one and fails in another, and sometimes a solution prepared with every precaution has failed to afford me any result. The two essential conditions to success are complete saturation and temperature. I have never succeeded with a temperature above 120°, nor with one below 102°; and so sensitive is the solution to moisture, that preparations sealed in it with asphalt seldom keep longer than one or two days, except in very dry weather. On a damp day I have known a successful preparation left on the object-glass disappear in six hours. The corneal mass may be kept unaltered for at least some weeks in the solution by running sealing-wax round the stopper of the bottle.

A perfectly successful preparation shows nothing but the cells. Unsuccessful preparations, especially those prepared with too hot solution, show globular masses unlike any anatomical element; others, especially those prepared at too low a temperature or with imperfect saturation, show masses of hexagonal crystals like those of cystin.

To sum up, I believe that there exists in the cornea:—

I., the fibrillary ground-substance, which is pierced by straight canals and honeycombed with cavities;

II., flat cells, which everywhere cover the fibrillary bundles of the former and line the entire system of the latter;

III., the cornea-corpuscles of Toynbee and Virchow; and,

IV., the nerve-structures of the tissue.

The cornea-corpuscles and the nerves lie free in the canals and cavities, and between them and the epithelium there is a fluid-filled space which permits the passage of lymph-corpuscles.

It is therefore proper to regard the canals, cavities, and inter-fibrillary spaces as forming a continuous and integral part of the lymphatic system, the latter having to the former the same relation that blood-capillaries have to the veins.

The junction of the flat cells of the fibrillary substance with the epithelium of the surface justifies the inference that the intercellular spaces in the anterior epithelium of the cornea communicate with the lymph-spaces in the ground-substance, and that the position of nerve-fibrillæ between the epithelium is a continuation of the similar relation that has been demonstrated in the substance of the structure.

It is a reasonable hypothesis that what can be definitely established for the cornea holds good for the other forms of connective tissue.

I have accordingly submitted tendon to an examination by different methods, with the view of obtaining evidence of the existence in that structure of cells other than those arranged longitudinally between the bundles, the nature of which has lately been carefully investigated by Boll, Spina, Ranvier, and others.

If the tendo Achillis of a frog, or the tendons of a mouse's tail, fixed according to the ingenious method described by Ranvier in his first paper*, are treated by nitrate of silver, care being taken to avoid friction of any kind, it is found that every part of the free surface of the bundles is covered by a continuous epithelium. In the tendo Achillis of the frog I have seen lymphatic capillaries distributed over this surface; and the epithelial markings can be traced from the cells covering the bundles into those of the vessels. A preparation from the tail of the mouse, showing this epithelium, is represented in fig. 4, Plate IX.

If sections of tendon are placed for several hours in a strong solution of extract of logwood and alum, and the dye then washed out by concentrated acetic acid, it is found that while the fibrillary substance becomes clear and transparent, the nuclei retain their colour. This is best done under a cover-glass and under the microscope, as the effect of the acid, if kept too long in contact with the preparation, is to discolour the nuclei also; the weight of the covering-glass is sufficient to prevent the otherwise invariable distortion of the preparation by the acid. If the preparation is intended to be permanent, all traces of the acid must be removed by a current of distilled water.

The effect of this treatment is to show that there exists in tendon a

* Archives de Physiologie, 1869.

far greater number of cells than can be seen in the most successful gold preparations. The figures illustrating the structure of tendon usually given by investigators account for only a portion of the cells whose existence is thus proved—that portion, namely, which consists of the rows of cells occupying the stellate spaces, and which colour easily in gold and carmine. In longitudinal sections, prepared by the method I have above described, not only are the nuclei much crowded together, but two are frequently seen on the same level, and applied to the opposing surfaces of contiguous bundles. In transverse sections a similar arrangement is found. The nuclei between the bundles are very numerous; two are often found together on opposite bundles; and in one stellate space three and four nuclei can often be found at the same level.

This is clearly a condition to which the so-called division of the nucleus is not applicable.

If we believe that each of these nuclei represents a cell, the conclusion is inevitable that, in addition to the cells hitherto described and occupying the centre of the stellate spaces, there exists another and very numerous class of cells applied to the surface of the bundles.

This effect of hæmatoxylin and subsequent action of acetic acid on sections of tendon is perfectly analogous to that similarly produced in sections of cornea.

The treatment of tendon by the potash solution has seldom yielded me satisfactory results; but when it has succeeded, I have found confirmation of the inferences I draw from the effect of the saturated solution of hæmatoxylin. A reference to figure 7 (Plate IX.) shows that while many of the cells isolated by the potash correspond to those found on the surface, others are similar to the long narrow cells that cover the fasciculi of fibrillary tissue in the cornea, and do not resemble in shape, even approximately, the superficial elements defined by nitrate of silver. Although I have not succeeded, as in the case of the cornea, in reducing tendon to a mass of these cells, I consider it a fair inference that the long narrow cells I have seen are samples of cells that invest the fasciculi of fibrillary tissue.

The comparative difficulty in successfully treating tendon by potash is probably due to the denseness of its structure.

It is in regard to the branched cells, which I hold to be the analogues of the branched cornea-cells (corpuscles), that the important fact demonstrated by Spina, that it is on the surface of the cells that the fibres of elastic tissue are formed, specially applies.

In the centrum tendineum of the rabbit the continuity of the flat cells, which in silver preparations are considered to indicate lymphatic vessels, with cells covering the fibrillary substance can be shown to a greater or less extent, according to the success which has attended this difficult manipulation. That it often succeeds in patches, is shown by

the plates that illustrate works on this subject, although the influence of Von Recklinghausen's doctrine (namely, that wherever an epithelium is found a lymphatic capillary must be supposed to exist) has led to what I believe to be their true nature being overlooked.

From a similar cause to that encountered in tendon, the complete reduction of the dense corium of mammals by potash is very difficult; but by treating thin sections of fresh cutis, isolated flat cells are found.

In the cutis of the frog, the bundles of fibrillary tissue are arranged in parallel layers, and the corium being thin, the demonstration of flat cells is easier. And here the continuity of these cells with those of the rete Malpighi is evident, in the same way as the cells of the anterior epithelium of the cornea are continuous with the flat cells of the interior of the structure.

Figures 11 and 12, Plate X., represent flat cells from the skin of the ox and frog.

I make the same inference in regard to the communication of the spaces between the cells of the rete Malpighi with the lymph-spaces of the corium, that I make in regard to the similar arrangement in the cornea, both as regards anatomical continuity and in regard to the position of the nerves in the spaces. Langerhans has described the network of nerve-fibres in the rete Malpighi, and, in that of the cutis of the rabbit, the rich network in the spaces between the cells is not very difficult to demonstrate. In the skin of the finger, I have traced a medullated nerve as high as the third layer.

Ranvier, in that part of his essay which treats of the *éléments cellulaires du tissu conjonctif lâche*, describes an entirely different anatomical element from that on which the authorities with whom he is in controversy had fixed their attention. The cells figured by him* are the same as those isolated by potash when very thin pieces of skin or areolar tissue are operated on. As described by him, they are applied closely to the bundles. But when he attempts to show that the connective-tissue corpuscle of Virchow does not exist, and that the appearances by which it is distinguished depend on an optical delusion, I believe him to be mistaken. In skin and subcutaneous tissue the chloride of gold brings out in the clearest manner the existence of nucleated cells with long projecting processes stretching between and around the bundles, the whole of the cells being connected by the anastomoses of their processes. So complete is the analogy between skin and tendon, that it would be easy to find parts of a successful gold preparation of skin where the diagnosis between skin and tendon might be difficult.

Figures 13 and 14, Plate X., illustrate the appearances presented by the branched cells in skin.

A history of the opinions held regarding the structure of the connective tissues since the time of Schwann is equally beyond the scope of

* *L. c.* p. 483.

this paper and my acquaintance with the literature of the subject. It is, however, well known that while twenty years ago the so-called connective-tissue corpuscles were believed to be concerned in the formation of elastic tissue, with the development of Virchow's doctrine of cellular pathology, this opinion seems to have been gradually abandoned, even by those who, like Virchow himself, had originally maintained it. Ranvier, whose investigations seem to have been conducted in singular independence of contemporary theories, holds that the first step in the appearance of elastic tissue is the formation of "*granulations réfringentes,*" traceable in the fully developed fibres.

In the spring of 1873, while investigating the structure of the touch-corpuscles of the finger, I found that the much-discussed cellular elements of these bodies, which colour in gold and carmine, anastomose with each other by means of fibres that resist prolonged maceration in concentrated acetic and dilute mineral acids, and I described them, in the account I gave of the results of my investigation, as "elastic tissue fibres." At the same time I found that similar cells and fibres form a thick network in the corium. Simultaneously, Spina made his exhaustive study of the connexion of the elastic fibres in tendon with the walls of the cell, to which I have already referred.

Since that time, I have continued to subject the skin and subcutaneous tissue to treatment by different methods, and the results have been confirmative of those I obtained in Vienna. Shortly expressed, the conclusion I have come to is, that, in skin, all the branched cells form elastic tissue on their surface and on their processes, and that there is no elastic tissue anywhere that is not so formed.

The cells found in connective tissue are divisible, as I believe, into two distinct classes. There are, first, the flat cells which never branch, and which, when treated by nitrate of silver, present appearances identical with those produced when the flat cells of serous membranes are similarly treated; secondly, there is the system of branched cells in its various forms. As contrasted with each other, they may be described simply as the flat and branched cells of connective tissue*. Between these two classes of cells there is no transition and no anatomical continuity. The forms of the branched cells embrace all the gradations between the fine network of a lymphatic gland and the anastomosing network of the strong fibres in skin and tendon. They are distinguished by their processes, their capacity to form a substance that resists acetic acid—the power, namely, of forming the resisting element specially characteristic of elastic tissue. That they do not all exercise this latter power to the same degree, does not constitute a sufficient difference to make it necessary to regard them as separable into classes essentially distinct.

The ligamentum nuchæ may be taken as the type of the stronger forms

* To flat cells the term *placoids* has been applied by Dr. Burdon Sanderson, the equivalent of the German *platten*.

of elastic tissue; and I select it for this reason to prove the cellular origin of its fibres.

If a thin piece of the ligamentum nuchæ is strongly coloured by chloride of gold and gently teased in glycerine, there will be found a number of oval nuclei lying loose amongst the fibres. But careful examination shows similar nuclei still adhering to many of the latter; and in some instances the remains of the protoplasm of the cell can be seen around the nucleus and adherent to the fibre. The nucleus and cell-remains are often found at the point of the division of a fibre into two, and indicate the original processes of the cell in the embryonic state.

If a portion of the same gold-stained ligament is further placed in a very strong solution of hæmatoxylin and alum for twelve hours, and then carefully spread out for examination, the appearances will be found to have considerably changed. If the preparation has not been roughly handled, the astringent effect of the latter solution has caused the clear outlines of the individual fibres to disappear, and, in their stead, there are flat bluish bands in which fine dark lines connecting oval swellings are seen. The latter are the nuclei, and the lines are permeable canals in the elastic fibres, which have become filled with the hæmatoxylin solution. Both these conditions are depicted in figures 15, 16, and 17, Plate X.

The formation of the elastic substance on the surface of the cell, as described by Spina in tendon, applies universally, and also holds good for the cell-processes. But the part of the cell-body that does not enter into the formation of this resisting substance, so far from sharing the strength of the new tissue, becomes more easily disintegrated than at an earlier period of its development, and can be found only when the tissue is cautiously manipulated. But sufficient staining with gold, and care in operating, will demonstrate the cellular origin of elastic fibres in whatever tissue they occur.

Virchow, as is well known, vindicates for his connective-tissue corpuscles the character of a connected chain of plasmatic canals, and I have remarked above regarding the tubular nature of the fibres of the ligamentum nuchæ. That every elastic fibre is permeable to fluid is highly probable, though not yet proven. This tubular nature of the larger fibres has produced one of the difficulties of the recognition of the connexion of the fibre with the cell. The chloride of gold colours the protoplasm of a cell, with which a fully developed fibre is continuous, a faint purple; and when the tinting is continued into the process, it is the contents of the tubular space that are coloured. The elastic fibre, unless carefully examined in a good light, is apt either to escape observation, or seems to run past the cell without being in continuity with it.

This difficulty has been increased by a chemical difference between the cell and the elastic tissue to which it gives origin, so that many reagents and modes of treatment, that by potash-lye for instance, dissolve the cell but leave the fibre untouched. Hence the methods that have been most

used for establishing the individual characters of elastic tissue have been instrumental in producing erroneous notions as to its origin.

Thus we have in skin, as in tendon, bundles of fibrillary tissue everywhere covered with flat cells, and, in the interstices of the bundles, the analogues of the branched cells of the cornea, producing a ramifying network of elastic tissue.

In gold preparations of the skin, the blood-vessels and nerves can be followed between the larger fasciculi, analogously to the position of the nerves in the cornea.

Fascia differs from skin and tendon only in so far as its flatness permits and necessitates a change of form in the flat cells, and the easy study of their arrangement and nature by nitrate of silver. If a half per cent. solution is injected under the skin of a mouse's back and the animal killed in from five to ten minutes afterwards, and the skin of the back dissected off, the fascia which has been in contact with the silver is recognized by its milky whiteness and œdematous condition. If spread out carefully on the object-glass in glycerine and exposed to sunlight, it is seen to be plated over with oblong or slightly rounded cells with large nuclei. Figure 8, Plate IX., is a sketch from a part of a preparation so obtained. The cells separated from the same structure by potash are represented in figure 9, Plate IX.; it will be observed that they are identical in appearance. Figure 10, Plate IX., illustrates the very large flat cells, with their nuclei, that cover the fascia of the muscles of the thigh of the frog.

Frequently, but not so constantly, the branched cells are also stained by the silver, and they are generally found at a different focus.

Ranvier ('Archives de Physiologie') has described flat cells on the sheaths of nerve-fasciculi and the investing membrane of nerve-bundles as constituting a lymphatic sheath.

By means of the saturated potash solution I have been able to satisfy myself that, not only are the nerve-bundles surrounded by lymphatic sheaths, but that each medullated nerve-fibre is invested with a layer of flat cells. This layer is closely applied to the medulla, and is internal to the sheath of Schwann. It is composed of extremely fine and delicate cells, and their demonstration by potash succeeds less frequently than does that of the cornea-cells; they are (as far as I have seen) without exception long and narrow, often tapering to an exceedingly fine point. In the finest forms their cellular nature is only to be distinctly made out by a magnifying-power of 700 or 800 diameters. Figure 18, Plate X., represents varieties of these cells and their relation to the medulla. Their length varies from 0·075 to 0·036 millim.; many of them are not more than 0·0015 millim. broad. Appearances are sometimes seen that would seem to indicate that the sheath of Schwann (tubular membrane) is lined by a layer of flat cells, distinct from that covering the medulla (white substance). The medulla, when treated by potash, presents

a series of bulgings, so that its lateral (optical) borders are designated by irregularly waving lines. One set of delicate cells can be seen closely following the sinuosities of the substance, while another set, more external, lie in a straight direction parallel to the longitudinal axis of the fibre. Where the medulla is constricted, there is a clear space between these two sets of cells, which are in contact at the convexities formed by the bulgings.

By adhering to the broad principle that wherever there is a nucleus there is a cell, the existence of a great number of cells surrounding the medulla of a nerve-fibre can be demonstrated in another way. If a nerve is placed in absolute alcohol for twenty-four hours and then very gently disentangled from the sheath in glycerine, a cover-glass put on and solution of hæmatoxylin drawn through the field by filter-paper, the nuclei of the fibres stain first, and their number soon becomes very striking. If the field is allowed to become saturated and obscure with the dye, and then subsequently cleared up by acetic acid, those fibres which have not suffered by the manipulation are literally dotted over with nuclei. The number is so great as at once to dispel the idea that they can be accounted for by the sheath of Schwann. The nuclei of the sheath can often be distinguished from the others by their more external position relative to the nerve and a deeper tint. Figure 19, Plate X., is drawn from a preparation made in this way.

The ring which, as Ranvier was the first to show, snares the medullated fibre is well seen when the nerve is treated by absolute alcohol or the saturated potash solution, both of which leave the medulla untouched. As at the seat of this constriction the medulla is deficient, and as the nerve-fibre is bathed in lymph, it is evident that there must be at these points a very intimate connexion between the lymph-fluid and the axis-cylinder of the nerve. This has been already indicated by Ranvier in his essay on the lymphatic nature of the nerve-sheaths, and receives greater force now that we know that flat cells, indicative of lymphatic structures, are situated on the fibres themselves.

The use of hæmatoxylin is as advantageous in demonstrating the large nuclei of the flat cells of the nerve-sheaths as it is in showing those of tendon.

Ranvier has observed that in transverse sections of nerves the sheaths and connective tissue surrounding the fibres stain more deeply with carmine than the surrounding tissue does. I have made a similar observation in the nerves of the skin in gold preparations which had been macerated in acetic acid. In this case the concentrically arranged connective tissue of the nerves is conspicuous by its pearly whiteness. But as we know that the surrounding corium is equally rich with the nerve in lymphatic structures, the cause of the difference in colour must be sought elsewhere, and will probably be found in relative differences in regard to the arrangement of the elastic tissue.

By combining several methods of investigation, I believe I have succeeded in clearing up some points in the anatomy of muscular fibre, by which it will be seen that, as regards the lymphatic system, muscle occupies a position almost identical with tendon and connective tissue generally.

If fresh muscle is deeply stained with hæmatoxylin and then treated by acetic acid and gently teased, the perimysium of the bundles and its very fine continuation around each fibre are seen to be studded with large round nuclei, which are far more numerous than those of the branched cells, which are also seen. The round nuclei belong to the flat cells of the perimysium.

I have been able to demonstrate the character of the cells by teasing the living muscle of the frog in aqueous humour, and thence transferring the separated fibres to the nitrate-of-silver solution. The usual sinuous lines are then seen both on the general and special perimysium. This is represented in figures 5 and 6, Plate IX.

When muscle is treated by the saturated solution of potash, as above described, the fibres are found unaltered, the striated appearance being well marked. There is no vestige left of the perimysium. On the naked surface of the sarcolemma, a number of round distinct nuclei are seen; and when they happen to be on the edge of the fractured fibre, it is seen that they are situated on its outer surface.

If the saturated potash solution in which the muscle is placed is kept for about an hour at a temperature of 110° Fahrenheit, and then allowed to cool gradually, we find a further effect has been produced.

On breaking down a piece of the muscle into its individual fibres, we find that although some of these are unaltered, others have lost all their nuclei, and present the appearance of a coarse granular cylinder. But there is sometimes a transition stage seen of peculiar interest. On the surface of the fibre the outlines of a series of quadrangular cells make themselves visible, each cell having a distinct nucleus; and it is easy to satisfy one's self that the nuclei of the cells are identical with the nuclei seen previously distributed over the surface of the sarcolemma. These cells are sometimes also seen free in the solution, in which case they are generally more or less broken up, but sometimes they are seen isolated in perfect condition. Figure 21, Plate XI., shows the cells becoming demarcated on the fibre, and figure 22, Plate XI., their appearance when isolated entire. The sarcolemma is sometimes seen freed both from the cells and their contents; and in this case the striped cylinder which may be seen near it is beset with small perforations.

The sarcolemma of muscle is thus covered with flat cells, regular in appearance and outline, which resist the action of a saturated solution of potash.

But the action of the potash teaches us something more. A fibre is sometimes found apparently unaltered, smooth in its contour, and still showing something of the striated appearance, but showing no nuclei.

One or more round holes are, however, seen on the pieces of broken muscle-cylinder, the more conspicuous because the nuclei are absent; they are about the size of the blood-corpuscle of the frog. By changing the focus, it is seen that each hole is only on one side of the fibre. The sharp clearness of their outline shows they are not artefacts, but spaces in which the sarcolemma is wanting (figure 23, Plate XI.). A further action of potash is seen when a muscular fibre is found channelled with one or more canals parallel to the long axis of the fibre. The canals thus seen are uniform in breadth; they are formed by rows of vacuoles, which correspond in shape and size to the nuclei of cells. (I had observed in studying the cornea that the first stage of the destruction of the flat cells, in the potash solution, is a vacuole taking the place of the nucleus.) By changing the focus, it is seen that these channels are in the substance of the fibre. Smaller channels and single vacuoles are seen in different planes.

A more extended degree of the action of potash on a fibre is when the central canal has no longer sharp outlines and is seen to contain granular débris.

Treatment of muscular fibre by hæmatoxylin gives, as regards nuclei, results confirmatory of those got by potash, in so far as a still greater number of nuclei are seen internal to the sarcolemma than is indicated even by that method. To obtain the best results from hæmatoxylin, the fibres should be isolated before being dyed. The excess of colour being removed by acetic acid, the nuclei become distinct; they are seen to be arranged in long rows, those of one row being in the same plane. Isolated nuclei are seen in different planes. An idea of their number is best formed from the appearance presented by the broken end of a fibre when it is turned upwards, giving a view equivalent to a transverse section. The whole thickness of the fibre is then seen to contain nuclei, in the arrangement of which something of a concentric disposition can generally be observed. The nuclei are large and oval, and contain one or two distinct nucleoli. If the substance of the fibre has been teased, it is seen that the fibrillæ are arranged in bundles which have an equal thickness, and isolated nuclei are seen adhering to their surface.

The inferences that are irresistible from these appearances prepare the way to readily understanding the more decided effects of an appropriate treatment by chloride of gold. The conditions of a successful examination of a muscular fibre by gold include the detachment of the perimysium from the fibre without injuring the latter, the obtaining good transverse views in the preparation, and the requisite degree of colouring. As it is impossible to ensure beforehand a combination of these favourable conditions, it is evident that, with equal care, success is not uniform. The results which I now give were obtained by teasing the muscles of the thigh of the frog in aqueous humour before staining with gold.

In the most perfect preparations thus obtained, the structure of a muscular fibre is seen to be almost identical with that of a fasciculus of

tendon. Longitudinally the fibre is seen to consist of parallel bundles of uniform width, separated by spaces that are indicated by distinct lines; and, distributed at intervals in the lines, are oblong nuclei, the long axis of which is parallel to that of the fibre. The breadth of the bundles is about the same as that of a secondary bundle in tendon; their surface is smooth and homogeneous (figure 25, Plate XI.).

A transverse view, corresponding to that of a cross section of tendon, shows the muscular substance intersected by stellate spaces, in some of which nuclei are seen, and, branching out from the spaces, a rich anastomosing network of fine dark lines divides the substance of the fibre into a number of compartments. Between the appearance I have just described and that of a cross section of tendon similarly prepared, the only difference is that, in muscle, the fields enclosed by the dark lines are dotted over by minute points, which may indicate the fibrillæ.

Nuclei are always seen in fibres successfully stained with gold, and especially when the fibre is separated by teasing into the bundles of fibrillæ; but their number is much less than that seen when hæmatoxylin is used. We have seen how, in the cornea, gold when it has deeply stained the nucleus of the branched cells leaves that of the flat cell invisible, while hæmatoxylin colours them both. So it is generally in the capillaries of blood-vessels. I have found that, in the capillaries of the muscles of the frog, these invariably consist of two layers—an internal epithelial layer, the outlines of whose cells are defined by nitrate of silver, and an external layer, into which a fine system of branched cells enters. Hæmatoxylin brings out the nuclei of the cells of both layers. The deep staining with gold, while it differentiates the layers by staining the internal (epithelial) more intensely than it does the outer (adventitious) layer, shows no nuclei in the epithelium, while the nuclei in the outer layer are well marked.

In applying to muscular fibre the experience thus acquired, we are warranted in concluding that the nuclei coloured in gold are those of cells that belong to the branched system, and which are the characteristic nuclei seen in the transverse view of a gold-stained muscle, while the great majority of those stained by hæmatoxylin belong to the flat cells of the lymphatic system.

The isolation of these cells is surrounded by difficulties, which are, however, surmountable. In fibres deeply stained by gold I have isolated long thin flat cells, lying amongst the fibrillæ, which are identical in shape with similar cells in the cornea. They were coloured uniformly deep purple, and a distinct nucleus was not visible. They are represented in figure 27, Plate XI.

Immediately investing the bundles composing a muscular fibre is the sarcolemma, which is externally, as I have shown, covered with flat cells. The property possessed by this membrane of resisting acetic acid is the cause of a characteristic appearance presented by a muscular fibre under

its influence. From the broken end a large uneven mass protrudes with thick everted lips, bending back over the membrane which forms a strangulating band round the neck of the protrusion. When this sheath is ruptured at different parts, the gelatinous substance, which forms a large proportion of the contents of the fibre, bulges out in masses as it swells. The fibrillæ, which do not swell under the acid, and which are imbedded in this mass, can be often seen, in teased gold or hæmatoxylin preparations, lying unaltered at one part of the field, while displaced masses of gelatinous substance are seen at another. (It is the disposition of this gelatinous substance in parallel bundles which is the cause of the peculiar effect of chloride of gold, represented in figure 25, Plate XI.)

The astringent effect of chloride of gold on the sarcolemma produces a very characteristic appearance. In manipulating a fibre, as a preliminary to its being hardened by gold, it sometimes happens that the membrane and the layer of muscle-substance adhering to it is rent longitudinally from the surface to the centre. In the gold solution it loses its cylindrical form, and spreads itself out as a broad band. This perfectly flat band is marked longitudinally with parallel lines, which are straight and equidistant from each other. The prolonged action of acetic acid does not alter the appearance of these lines or their mutual relations, but it makes visible a not very thick layer of gelatinous substance, which protrudes from under the edges of the band.

Without comparing this peculiar appearance in its most exquisite forms with the transition stages sometimes seen, in which one end of a fibre still retains its cylindrical form while the other end is flattened out, the observer might certainly doubt that he was looking at a muscular fibre. Interstices between the lines, and, in them, occasional oblong nuclei are sometimes visible.

The longitudinal lines are the optical expression of the septa between the bundles, which are seen through the transparent sheath; and that the fibres in these septa are formed by elastic tissue, is shown by their persistence when treated by acetic acid.

They differ in no respect from the septa and their contained nuclei, which are seen in muscular fibres that have retained their cylindrical form when the chloride of gold has produced that appearance.

Another occasional effect of the astringent action of gold is an exaggeration of the dimensions of the central canal. The upturned end of a fibre is sometimes seen in which there is the appearance of a wide central cavity, around which the contents of the sarcolemma form a thick rim. The mechanism of this appearance is explicable by the assumption that the sarcolemma becomes sufficiently unyielding to form an immovable surface, towards which the more yielding substance is drawn as the shrinking caused by the gold proceeds.

The sarcolemma is probably in very intimate connexion with the elastic network, the more superficial cells of which, with their prolongations, are

situated directly under and apparently in contact with it; and the numerous foramina seen in the cylindrical rod left by the potash solution, when the membrane has been loosened from it, are probably the points where the elastic fibres penetrate.

A muscular fibre is thus composed of a number of bundles resembling those of tendon, arranged parallel to each other, each bundle giving shelter to a number of fibrillæ, and separated from the neighbouring bundles by a space lined with flat cells. In the larger spaces lie branched cells, and in the smaller the projecting processes of the elastic fibres given out by the latter.

The large holes I have described in the elastic sheath afford passage to the nerves. When these have been traced, it is reasonable to infer that, here as well as elsewhere, they will be found to follow the lymph-channels. These holes not only permit the passage of nerves, but allow free communication between the lymphatic spaces within the fibre and those between the perimysium and the sarcolemma.

The abundance of gelatinous substance in a muscular fibre accounts for the phenomenon known as transverse cleavage, which is produced by the action of very diluted hydrochloric acid. I regard it as essentially equivalent to the effect produced in tendon when by similar treatment a bundle divides transversely into the flat plates known as the "Donderische Bänder," after the distinguished histologist who first described them.

To sum up these views regarding the structure of muscle in a few words, it might be said that a muscular fibre is a fasciculus of tendon in the bundles of which the primitive fibrillæ are imbedded longitudinally.

The small spaces at the points of junction of flat cells which colour deeply in silver, and to which allusion has been made by histologists, are seen in all tissues. They are always present when the colouring has been intense, and should, I believe, be regarded as playing an important part in the mechanism of the lymphatic system. They are especially well defined in the rete Malpighi of the frog, where it would be impossible to regard them as artefacts.

It is evident from the various anatomical facts above detailed, that the tissues may be said to be in an almost unbroken continuity with the lymph-system*. When a blood-corpuscle escapes from a capillary it is into the cell-lined spaces of this system that it directly passes, and there is manifestly no obstacle to the passage of the contents of the lymph-channel into the blood other than that formed by the wall of the capillaries, which alone separates the fluids of the lymphatic and vascular systems. We know that white blood-corpuscles can make their way

* In this connexion I quote from Ranvier's essay (*l. c.* p. 485) the following sentence:—"L'existence, dans le tissu cellulaire sous-cutané, de ces cellules plates, disposées à la surface des faisceaux, ne nous suggère-t-elle pas l'idée de voir dans le tissu conjonctif un vaste espace cloisonné, analogue aux cavités séreuses?".

through at the points of junction of the angles of the capillary cells*, and it is reasonable to suppose that these points are always permeable to fluids.

We have seen that there is a rich supply of lymphatic channels in the interior of a muscular fibre, and that the axis-cylinder of a nerve is probably in free communication with the lymph. The term "invagination," as applied to the relation of the nerves and blood-vessels of particular organs to the lymphatics, has no special physiological meaning, as it only implies that at certain parts a condition that is universal can, by special modes of procedure, be made capable of more easy demonstration. Every nerve-fibre and every blood-vessel is invaginated in lymphatics.

That there is a plasmatic circulation infinitely more comprehensive than that expounded by Virchow, is, as has been already remarked by Ranvier, a fact which anatomy has placed on an incontrovertible basis.

EXPLANATION OF THE PLATES.

The Drawings were executed by Mr. J. C. Ewart from my preparations. The objectives and ocular glasses referred to as indicating the magnifying-powers are those of Hartnack, with the exception of the No. XII. immersion-lens used in a few instances, which was made by Vérick, and has the power assigned to that number in his scale. Thus 3. VIII. means eyepiece No. 3 and objective No. VIII.

Plate VIII.

Fig. 1. Cells from the cornea of a frog which was treated by the saturated solution of potash. 3. VIII. Tube out.

Fig. 2. Cells from the cornea of the ox treated by solution of potash. 3. VIII. Tube out.

Fig. 3. Cells from the cornea of the sheep treated by solution of potash. 3. VIII. Tube out.

Plate IX.

Fig. 4. Tendon from a mouse's tail coloured by nitrate of silver. 3. VII. Tube out.

Fig. 5. Perimysium of muscle of frog. Silver preparation. 3. VII. Tube out.

Fig. 6. Perimysium of a muscular fibre of frog. Silver preparation. 3. VII. Tube out.

Fig. 7. Cells from tendo Achillis of frog by solution of potash. 3. VIII. Tube out.

Fig. 8. Fascia from dorsal muscles of the mouse. Nitrate-of-silver preparation. 3. VIII. Tube out.

Fig. 9. Cells isolated from the fascia of the dorsal muscles of the mouse by solution of potash. 3. VIII. Tube out.

Fig. 10. Continuous layer of flat cells investing the fascia of the muscles of the thigh of the frog. Nitrate-of-silver preparation. 3. VIII. Tube out.

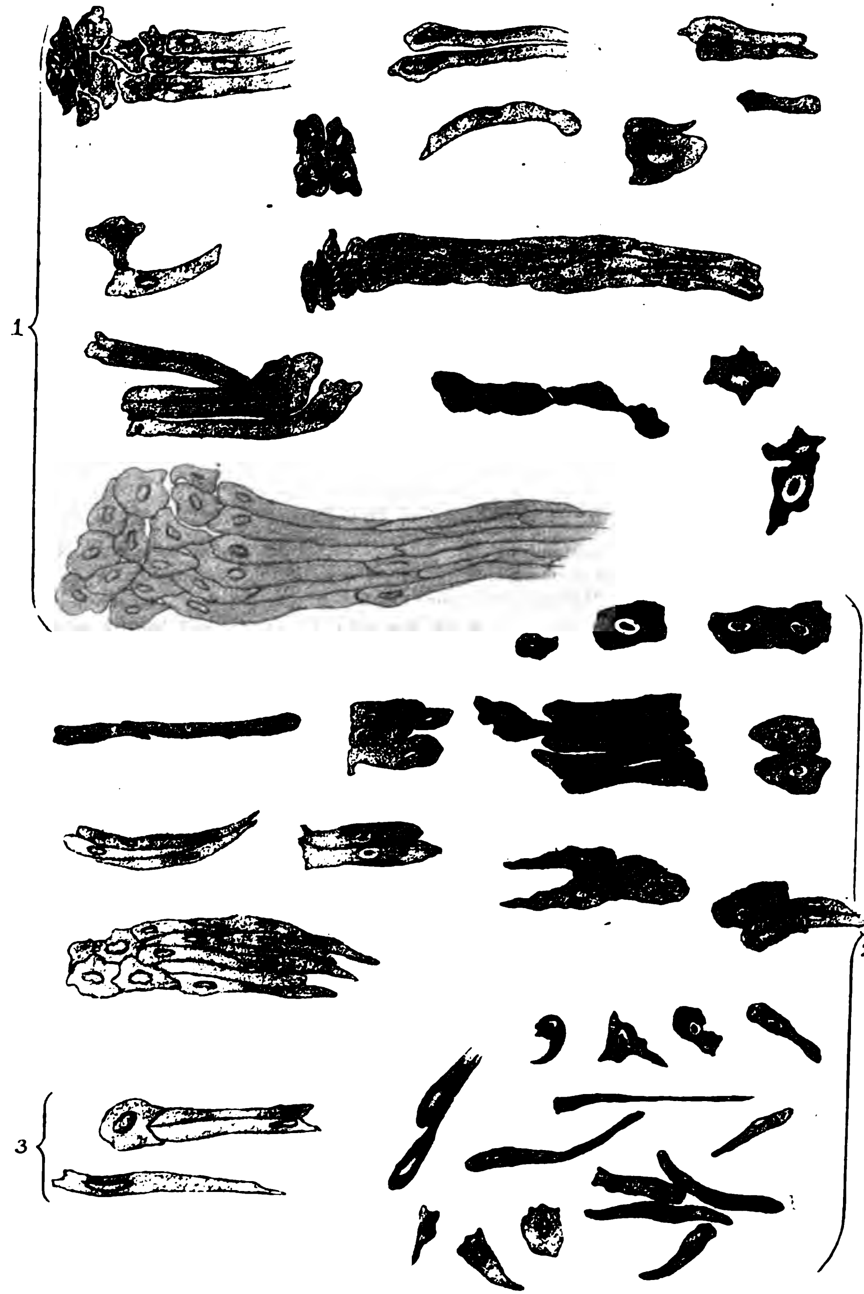
Plate X.

Fig. 11. Cells of the cutis of the frog isolated by solution of potash. 3. VIII. Tube out.

Fig. 12. Cells isolated from the skin of the ox by solution of potash. 3. VIII. Tube out.

Fig. 13. The anastomosis of the cells by means of the elastic fibres. Gold preparation from finger, macerated in acetic acid. 3. XII.

* Endothelium en Emigratie door Dr. Laidlaw Purves. Utrecht, 1873.



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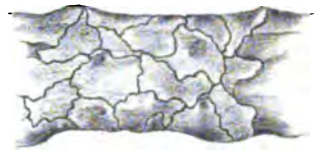

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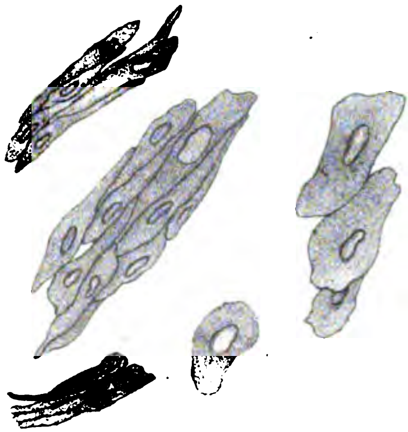

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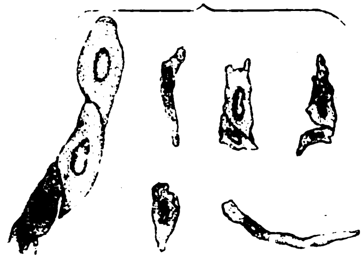

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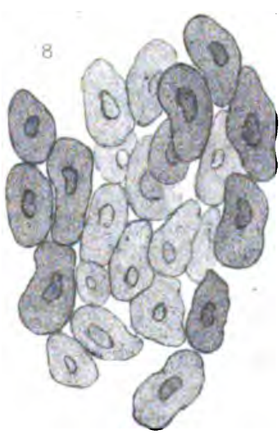
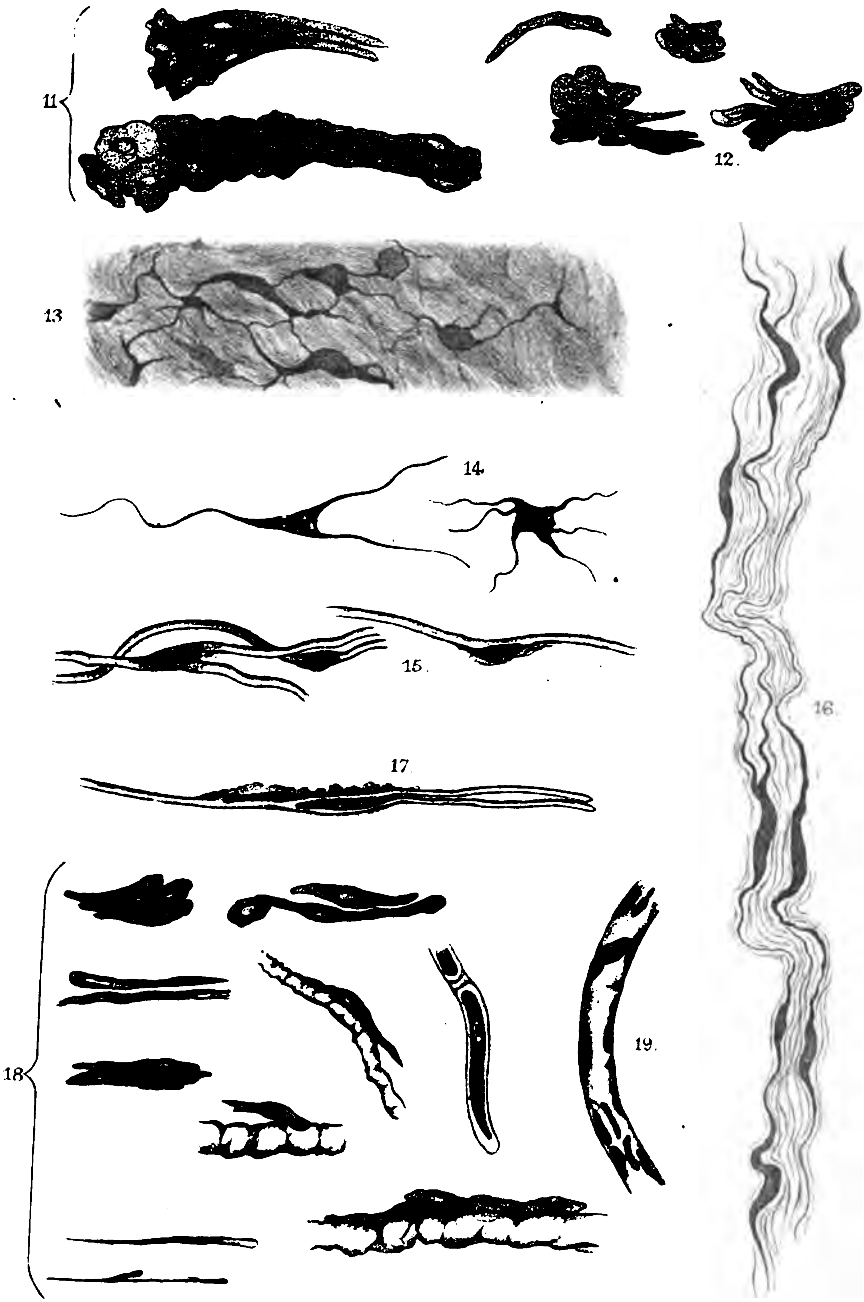


J.C. Ewart del. W.H. Wesley lith.

W. West & Co. imp.



J C Ewart del. W H Westley lith.

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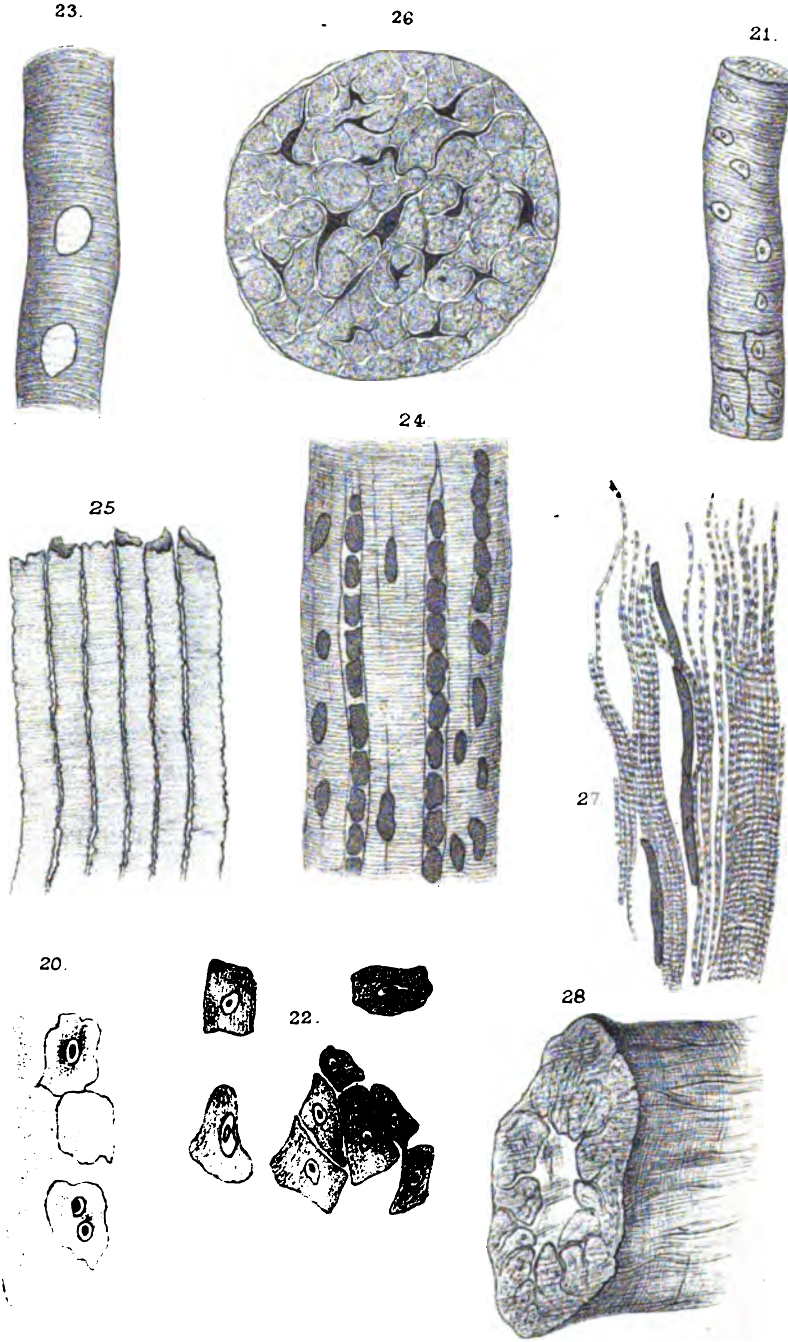

J. C. Ewart del. W. H. Wesley lith.   W. West & Co imp.

Fig. 14. Elastic fibres with cells. Section from gold preparation of skin of adult rabbit treated by concentrated acetic acid. 3. VIII. Tube out.

Fig. 15. Fibres from the ligamentum nuchæ of a three-days-old foal. Gold preparation. The nuclei and remains of the protoplasm of the cell stained. 3. VIII. Tube out.

Fig. 16. Ligamentum nuchæ of three-days-old foal stained in gold and hæmatoxylin. The central canal of the fibres indicated by the hæmatoxylin. 3. VIII. Tube out.

Fig. 17. Fibre from the same preparation as fig. 15. 1. XII. Tube out.

Fig. 18. Cells from the fibres of the sciatic nerve of the frog. Isolated by the saturated solution of potash. 3. VII. Tube out.

Fig. 19. Nerve-fibre from the sciatic nerve of the mouse. Treated by absolute alcohol, dyed with hæmatoxylin, and the excess of colour removed by acetic acid. 3. VIII. Tube out.

PLATE XI.

Fig. 20. Perimysium of a muscular fibre of frog stained in hæmatoxylin. Flat cells seen. 3. VII. Tube out.

Fig. 21. Muscle of mouse subjected to prolonged action of warm potash solution. The cells on the sarcolemma indicated. 3. VIII. Tube out.

Fig. 22. Flat cells from the sarcolemma of muscular fibre of ox. Isolated by prolonged action of warm potash solution. 3. VIII. Tube out.

Fig. 23. Muscular fibre of mouse treated by solution of potash. The holes in the sarcolemma seen. 3. VIII. Tube out.

Fig. 24. Muscular fibre of frog treated by solution of potash. Canals indicated by nuclear vacuoles. 3. VIII. Tube out.

Fig. 25. Muscular fibre of frog. Gold preparation. Sarcolemma rent longitudinally and flattened. Septa dividing muscular substance visible. 3. VII. Tube out.

Fig. 26. End view of muscular fibre of frog. Gold preparation. Stellate spaces with nuclei and processes of branched cells, having the signification of elastic fibres, between the bundles. 3. VII. Tube out.

Fig. 27. Muscular fibre of frog. Gold preparation. The fibrillæ separated by teasing into bundles, between which long narrow flat cells are seen. 3. VII. Tube out.

Fig. 28. Muscular fibre of frog. Gold preparation. The central cavity seen much enlarged by the astringent action of the gold. 3. VII. Tube out.

"On the Refraction of Sound by the Atmosphere." By Prof. OSBORNE REYNOLDS. Communicated by Prof. G. G. STOKES, Sec.R.S. Received March 18, 1874*.

My object in this paper is to offer explanations of some of the more common phenomena of the transmission of sound, and to describe the results of experiments in support of these explanations. The first part of the paper is devoted to *the action of wind upon sound.* In this part of the subject I find that I have been preceded by Professor Stokes, who in 1857 gave precisely the same explanation as that which occurred to me. I have, however, succeeded in placing the truth of this explanation upon an experimental basis; and this, together with the fact that my work upon this part of the subject is the cause and foundation of what

* Read April 23, 1874. See *antè*, p. 295.

I have to say on the second part, must be my excuse for introducing it here. In the second part of the subject I have dealt with the effect of the atmosphere to refract sound upwards, an effect which is due to the variation of temperature, and which I believe has not hitherto been noticed. I have been able to show that this refraction explains the well-known difference which exists in the distinctness of sounds by day and by night, as well as other differences in the transmission of sound arising out of circumstances such as temperature; and I have applied it in particular to explain the very definite results obtained by Professor Tyndall in his experiments off the South Foreland.

The Effect of Wind upon Sound

is a matter of common observation. Cases have been known in which, against a high wind, guns could not be heard at a distance of 550 yards*, although on a quiet day the same guns might be heard from ten to twenty miles. And it is not only with high winds that the effect upon sound is apparent; every sportsman knows how important it is to enter the field on the lee side even when the wind is very light. In light winds, however, the effect is not so certain as in high winds; and (at any rate so far as our ears are concerned) sounds from a small distance seem at times to be rather intensified than diminished against very light winds. On all occasions the effect of wind seems to be rather against distance than against distinctness. Sounds heard to windward are for the most part heard with their full distinctness; and there is only a comparatively small margin between that point at which the sound is perceptibly diminished and that at which it ceases to be audible.

That sound should be blown back by a high wind does not at first sight appear to be unreasonable. Sound is known to travel forward through or on the air; and if the air is itself in motion, moving backwards, it will carry the sound with it, and so retard its forward motion—just as the current of a river retards the motion of ships moving up the stream. A little consideration, however, serves to show that the effect of wind on sound cannot be explained in this way. The velocity of sound (1100 feet per second) is so great compared with that of the highest wind (50 to 100 feet per second), that the mere retardation of the velocity, if that were all, would not be apparent. The sound would proceed against the wind with a slightly diminished velocity, at least 1000 feet per second, and with a but very slightly diminished intensity.

Neither can the effect of wind be solely due to its effect on our hearing. There can be no doubt that during a high wind our power of hearing is damaged; but this is the same from whatever direction the sound may come; and hence from this cause the wind would diminish the distance at which sounds could be heard, whether they moved with it or against it, whereas this is most distinctly not the case. Sounds at

* Proc. Roy. Soc. 1874, p. 62.

right angles to the wind are but little affected by it; and in moderate winds sounds can be heard further with the wind than when there is none.

The same may be said against theories which would explain the effect of wind as causing a heterogeneous nature in the air so that it might reflect the sound. All such effects must apply with equal force with and against the wind.

This question has baffled investigators for so long a time, because they have looked for the cause in some direct effect of the motion of the air, whereas it seems to be but incidentally due to this. The effect appears, after all, not to be due simply to the wind, but to the difference in the velocity with which the air travels at the surface of the ground and at a height above it; that is to say, if we could have a perfectly smooth surface which would not retard the wind at all, then the wind would not obstruct sound in the way it does, for it would all be moving with an equal velocity; but, owing to the roughness of the surface and the obstructions upon it, there is a gradual diminution in the velocity of the wind as it approaches the surface. The rate of this diminution will depend on the nature of the surface; for instance, in a meadow the velocity at 1 foot above the surface is only half what it is at an elevation of 8 feet, and smaller still compared with what it is at greater heights.

To understand the way in which this variation in the velocity affects the sound, it is necessary to consider that the velocity of the waves of sound does depend on the velocity of the wind, although not in a great degree. To find the velocity of the sound with the wind we must add that of the wind to the normal velocity of sound, and against the wind we must subtract the velocity of the wind from the 1100 feet per second (or whatever may be the normal velocity of the sound) to find the actual velocity. Now if the wind is moving at 10 feet per second at the surface of a meadow, and at 20 feet per second at a height of 8 feet, the velocity of the sound against the wind will be 1090 feet per second at the surface and 1080 feet per second at 8 feet above the surface; so that in a second the same wave of sound will have travelled 10 feet further at the surface than at a height of 8 feet. This difference of velocity would cause the wave to tip up and proceed in an upward direction instead of horizontally. For if we imagine the front of a wave of sound to be vertical to start with, it will, after proceeding for one second against the wind, be inclined at an angle of more than 45°, or half a right angle; and since sound-waves always move in a direction perpendicular to the direction of the front (that is to say, if the waves are vertical they will move horizontally and not otherwise), after one second the wave would be moving upwards at an angle of 45° or more. Of course, in reality, it would not have to proceed for one second before it began to move upwards: the least forward motion would be followed

by an inclination of the front backwards, and by an upward motion of the wave. A similar effect would be produced in a direction opposite to that of the wind, only as the top of the wave would then be moving faster than the bottom, the waves would incline forwards and move downwards. In this way the effect of the wind is to lift the waves which proceeded to windward, and to bring those down which move with it.

Thus the effect of wind is not to destroy the sound, but to raise the ends of the wave, which would otherwise move along the ground, to such a height that they pass over our heads.

When the ends of the waves are raised from the ground they will tend to diverge down to it, and throw off *secondary waves*, or, as I shall call them, *diverging waves*, so as to reconstitute the gap that is thus made. These secondary waves will be heard as a continuation of the sound, more or less faint, after the primary waves are altogether above our heads. [This phenomenon of divergence presents many difficulties, and has only as yet been dealt with for particular cases. It may, however, be assumed, from what is known respecting it, that in the case of sound being lifted up from the ground by refraction, or, what is nearly the same thing, passing directly over the crest of a hill so that the ground falls away from the rays of sound, diverging waves would be thrown off very rapidly at first and for a considerable distance, depending on the wave-length of the sound; but as the sound proceeds further the diverging rays would gradually become fainter and more nearly parallel to the direct rays, until at a sufficient distance they would practically cease to exist, or, at any rate, be no greater than those which cause the diffraction-bands in a pencil of light*. The divergence would introduce bands of diffraction or interference within the direct or geometrical path of the sound, as in the case of light. These effects would also be complicated by the reflection of the diverging waves from the ground, which, crossing the others at a small angle, would also cause bands of interference. The results of all these causes would be very complicated, but their general effect would be to cause a rapid weakening of the sound at the ground from the point at which it was first lifted; and as the sound became weaker it would be crossed by bands of still fainter sound, after which the diverging rays, as well as the direct rays, would be lifted, and at the ground nothing would be heard.—*September* 1874.]

If we leave out of consideration the divergence, then we may form some idea as to the path which the bottom of the sound, or the rays of sound (considered as the rays of light), would follow. If the variation in the speed of the wind were uniform from the surface upwards, then

* Taking sound of 1 foot wave-length, and comparing it with light whose wave-length is the 50,000th part of an inch, then the divergence of the sound at a mile from the point at which it left the ground would be comparatively the same as that of the light at $\frac{1}{10}$ of an inch from the aperture at which the pencil was formed.

the rays of sound would at first move upwards, very nearly in circles. The radii of these circles may be shown to be $1100 \times \frac{h}{v_1 - v_2}$, where v_1 and v_2 are the velocities of the wind in feet per second at elevations differing by h feet. In fact, however, the variation is greatest at the ground, and diminishes as we proceed upwards, so that the actual path would be more that of a parabola.

Also, owing to this unequal variation in the velocity, those parts of the waves immediately adjacent to the ground will rise more rapidly than the part immediately above them; hence there will be a crowding of the waves at a few feet from the ground, and this will lead to an intensifying of the sound at this point. Hence, notwithstanding the divergence, we might expect the waves to windward to preserve their full intensity so long as they were low enough to be heard. And this is in accordance with the fact, often observed, that sounds at short distances are not diminished but rather intensified when proceeding against the wind.

It will at once be perceived that by this action of the wind the distance to which sounds can be heard to windward must depend on the elevation of the observer and the sound-producing body. This does not appear to be a fact of general observation. It is difficult to conceive how it can have been overlooked, except that, in nine cases out of ten, sounds are not continuous, and thus do not afford an opportunity of comparing their distinctness at different places. It has often astonished me, however, when shooting, that a wind which did not appear to me to make the least difference to the direction in which I could hear small sounds most distinctly, should yet be sufficient to cover one's approach to partridges, and more particularly to rabbits, even until one was within a few feet of them—a fact which shows how much more effectively the wind obstructs sound near the ground than even a few feet above it.

Elevation, however, clearly offered a crucial test whether such an action as that I have described was the cause of the effect of wind upon sound. Having once entertained the idea, it was clearly possible to put it to the test in this way. Also, if the principles hold in sound, something analogous must hold in the case of waves on the surface of a running stream of water—for instance, waves made near the bank of a river.

I had just reached the point of making such tests when I discovered that the same views had been propounded by Professor Stokes so long ago as 1857*. Of course, after such a discovery, it seemed almost unnecessary for me to pursue the matter further; but as there were one or two points about which I was not then quite certain, and as Prof. Stokes's paper does not appear to be so well known as it might be (I do not know of one writer on sound who has adopted this explanation), it still seemed that it might be well, if possible, to put the subject on an experimental basis. I therefore made the experiments I am

* Brit. Assoc. Report, 1857, Trans. of Sect. p. 22.

about to describe; and I am glad that I did not rest content without them, for they led me to what I believe to be the discovery of refraction of sound by the atmosphere.

Fig. 1.

The results of my first observation are shown in fig. 1. This represents the shape of the waves as they proceeded outwards from a point near the bank of a stream about 12 feet wide. Had the water been at rest there would have been semicircular rings; as it was, the front of the waves up the stream made an obtuse angle with the wall, which they gradually left. The ends of the waves, it will be observed, gradually died out, showing the effect of divergence. The waves proceeding down the stream were, on the other hand, inclined to the wall, which they approached.

I was able to make a somewhat better observation in the Medlock, near the Oxford Road Bridge, Manchester. A pipe sent a succession of drops into the water at a few inches from the wall, which, falling from a considerable height, made very definite waves. Fig. 2 represents

Fig. 2.

a sketch of these waves, made on the spot: the diverging waves from the ends of the direct waves, and also the bands of interference, are very

clearly seen. Both these figures agree with what has been explained as the effect of wind on sound.

In the next place I endeavoured to ascertain the effect which elevation has on the distance to which sound can be heard against a wind. In making these experiments I discovered some facts relating to the transmission of sound over a rough surface, which, although somewhat obvious, appear hitherto to have escaped attention.

My apparatus consisted of an electrical bell, mounted on a case containing a battery. The bell was placed horizontally on the top of the case, so that it could be heard equally well in all directions; and when standing on the ground the bell was 1 foot above the surface. I also used an anemometer.

These experiments were made on four different days, the 6th, 9th, 10th, and 11th of March. On the first of these the wind was very light, on the others it was moderately strong, strongest on the second and fourth; on all four the direction was the same, viz. north. On the two last days the ground was covered with snow, which gave additional interest to the experiments, inasmuch as it enabled me to compare the effect of different surfaces. On the first two days I was alone, but on the last two I had the assistance of Mr. J. B. Millar, of Owens College, whose ears were rather better than mine, although I am not aware of any deficiency in this respect. The experiments were all made in the same place, a flat meadow of considerable extent.

The General Results of the Experiments.

De La Roche*, in his experiment, found that the wind produced least effect on the sound at right angles to its direction, *i. e.* sounds could be heard furthest in this direction. His method of experimenting, however, was not the same as mine. He compared the sounds from two equal bells, and in all cases placed the bells at such distances that the sounds were equally distinct. I, on the other hand, measured the extreme distance at which the sounds could be heard, the test being whether or not the observer noticed a break in the continuity of sound, a stoppage of the bell. The difference in our method of experimenting accounts for the difference in our results. I found in every case that the sound could be heard further with the wind than at right angles to its direction; and when the wind was at all strong, the range with the wind was more than double that at right angles. It does not follow, however, nor was the fact observed, that at comparatively short distances the sound with the wind was more intense than at right angles.

The explanation of this fact, which was fully borne out by all the experiments, is that the sound which comes in immediate contact with the ground is continually destroyed by the rough surface, and the sound from above is continually diverging down to replace that which has been

* Annales de Chimie, vol. i. p. 177 (1816).

destroyed. These diverging waves are in their turn destroyed; so that there is a gradual weakening of the intensity of the waves near the ground, and this weakening extends upwards as the waves proceed. Therefore, under ordinary circumstances, when there is no wind the distant sounds which pass above us are more intense than those which we hear. Of this fact I have abundant evidence. On the 6th, when the wind was light, at all distances greater than 20 yards from the bell the sound was much less at the ground than a few feet above it; and I was able to recover the sound after it had been lost in every direction by mounting on to a tree, and even more definitely by raising the bell on to a post 4 feet high, which had the effect of doubling the range of the sound in every direction except with the wind, although even in this the range was materially increased.

It is obvious that the rate at which the sound is destroyed by the ground will depend on the roughness of its surface. Over grass we might expect the sound at the ground to be annihilated, whereas over water it would hardly be affected. This was shown to be the case by the difference in the range at right angles to the wind over grass, and over the same ground when completely covered with snow. In the latter case I could hear the sound at 200 yards, whereas I could only hear it at 70 or 80 in the former.

Now, owing to the fact that the sound is greater over our heads than at the ground, any thing which slowly brings down the sound will increase the range. Hence, assuming that the action of the wind is to bring down the sound in the direction in which it is blowing, we see that it must increase its range in this direction. And it must also be seen that in this direction there will be less difference in the intensity of the sound from the ground upwards than in other directions. This was observed to be the case on all occasions. In the direction of the wind, when it was strong, the sound could be heard as well with the head on the ground as when raised, even when in a hollow with the bell hidden from view by the slope of the ground; and no advantage whatever was gained either by ascending to an elevation or raising the bell. Thus, with the wind over the grass the sound could be heard 140 yards, and over snow 360 yards, either with the head lifted or on the ground; whereas at right angles to the wind on all occasions the range was extended by raising either the observer or the bell.

It has been necessary to notice these points; for, as will be seen, they bear directly on the question of the effect of elevation on the range of sound against the wind.

Elevation was found to affect the range of sound against the wind in a much more marked manner than at right angles.

Over the grass no sound could be heard with the head on the ground at 20 yards from the bell, and at 30 yards it was lost with the head 3 feet from the ground, and its full intensity was lost when standing erect at

30 yards. At 70 yards, when standing erect, the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised 9 feet from the ground, and it reached its full intensity at an elevation of 12 feet.

Over the snow similar effects were observed at very nearly equal distances. There was this difference, however, the sound was not entirely lost when the head was lowered or even on the ground. Thus at 30 yards I could still hear a faint sound. Mr. Millar could hear this better than I could; he, however, experienced the same increase on raising his head. At 90 yards I lost the sound entirely when standing on the ground, but recovered it again when the ear was 9 feet from the ground. Mr. Millar, however, could hear the sound very faintly, and at intervals, at 160 yards; but not with his head on the ground. At this point I was utterly unable to hear it; and even at an elevation of 25 feet I gave it up as hopeless. However, as Mr. Millar by mounting 10 feet higher seemed to hear it very much better, I again ascended; and at an elevation of 33 feet from the ground I could hear it as distinctly as I had previously heard it when standing at 90 yards from the bell. I could not hear it 5 feet lower down; so that it was the last 5 feet which had brought me into the foot of the wave. Mr. Millar experienced the same change in this 5 feet. As the sound could now be heard as strong as at a corresponding distance with the wind, we thought we had reached the full intensity of the waves. This, however, was not the case; for the least raising of the bell was followed by a considerable intensifying of the sound; and when it was raised 6 feet I could hear each blow of the hammer distinctly, although just at that time a brass band was playing in the distance. It seemed to me that I could hear it as distinctly as at 30 yards to leeward of the bell. All these results were repeated on both days with great uniformity.

When more than 30 yards to the windward of the bell, the raising of the bell was always accompanied by a marked intensifying of the sound, and particularly over the grass. I could only hear the bell at 70 yards when on the ground; yet when set on a post 5 feet high I heard it 160 yards, or more than twice the distance. This is a proof of what I previously pointed out, that the waves rise faster at the ground than they do high up, and crowding together they intensify. In all cases there was an unmistakable greater distinctness of the sound from short distances to windward than to leeward or at right angles.

Except when the sound was heard with full force it was not uniform. The bell gave two sounds (the beats of the hammer and the ring) which could be easily distinguished; and at times we could hear only the ring, and at others the beats. The ring seemed to preserve itself the longest; whereas near the ground at short distances the ring was lost first. This is explained by the fact that the rate at which sound-waves diverge depends upon their note: the lower the note the more will they diverge.

Thus the beats diverge more rapidly than the ring, and consequently die out sooner; whereas when the head is on the ground near the bell it is only the diverging waves that are heard, and here the beats have the best chance. The intensity of the sound invariably seemed to waver; and as one approached the bell from the windward side, the sound did not intensify uniformly or gradually, but by fits or jerks; this was the result of crossing the rays' interference, such as those shown in fig. 2.

During the observations the velocity of the wind was observed from time to time at points 1 foot and 8 feet above the surface.

On the 9th, that is over grass, it varied from 4 feet per second at 1 foot and 8 feet per second at 8 feet, to 10 feet at 1 foot and 20 feet at 8 feet, always having about twice the velocity at 8 feet that it had at 1 foot above the ground.

Over the snow there was not quite so much variation above and below. On the 10th the wind varied from 3 feet at 1 foot to 4 feet at 8 feet *. On the 11th the variation was from 12 at 1 foot and 19 at 8 feet to 6 at 1 foot and 10 at 8 feet. Thus over snow the variation in the velocity was only about one third instead of half.

Since the foregoing account was written, I have had an opportunity of experimenting on a strong west wind (on the 14th of March); and the results of these experiments are, if any thing, more definite than those of the previous ones. The wind on this occasion had a velocity of 37 feet per second at an elevation of 12 feet and of 33 at 8 feet and 17 at 1 foot. The experiments were made in the same meadows as before, the snow having melted, so that the grass was bare.

With the wind I could hear the bell at 120 yards, either with the bell on the ground or raised 4 feet above it. At right angles to the direction of the wind it ranged about 60 yards with the bell on the ground, and 80 yards when the bell was elevated.

To windward, with the bell standing on the ground (which, it must be remembered, means that the bell was actually 1 foot above the surface), the sound was heard as follows:—

	Full.	Lost.
With the head close to the ground..	At 10 yards.	At 20 yards.
Standing	„ 30 „	„ 40 „
At an elevation of 25 feet	*Not heard* at 90 yards.	

With the bell at an elevation of 4 feet 6 inches:—

	Full.	Lost.
Head to the ground..............	At 18 yards.	At 30 yards.
Standing up	„ 40 „	„ 60 „
At an elevation of 12 feet	....	„ 90 „
At an elevation of 18 feet	„ 90 „	

These results entirely confirm those of the previous experiments; and the intensifying of the sounds to windward by the raising of the bell was

* The wind fell rapidly towards the close of the observations on this day.

even more marked than before; for at 90 yards to windward, with the bell raised, I could hear it *much* more distinctly than at a corresponding distance to leeward. This fact calls for a word of special explanation; it is clearly due to the fact that the variation in the velocity of the air is much greater near the ground than at a few feet above it. When the bell is on the ground all the sound must pass near the ground, and will all be turned up to a nearly equal extent; but when the bell is raised, the rays of sound which proceed horizontally will be much less bent or turned up than those which go down to the ground; and consequently, after proceeding some distance, these rays will meet or cross, and if the head be at this point they will both fall on the ear together, causing a sound of double intensity. It is this crossing of the rays also which for the most part causes the interference seen in fig. 2.

These experiments establish three things with regard to the transmission of sound:—

1. That when there is no wind, sound proceeding over a rough surface is more intense above than below.

2. That as long as the velocity of the wind is greater above than below, sound is lifted up to windward and is not destroyed.

3. That under the same circumstances it is brought down to leeward, and hence its range extended at the surface of the ground.

These experiments also show that there is less variation in the velocity of the wind over a smooth surface than over a rough one.

It seems to me that these facts fully confirm the hypotheses propounded by Prof. Stokes, that they place the action of wind beyond question, and that they afford explanations of many of the anomalous cases that have been observed; for instance, that sounds can be heard much further over water than over land, and also that a light wind at sea does not appear to affect sound at all, the fact being that the smooth water does not destroy either the sound or the motion of the air in contact with it. When the wind and sea are rough the case is different.

The Effect of Variations of Temperature.

Having observed how the wind acts to lift the waves of sound by diminishing their velocity above compared with what is below, it was evident to me that any other atmospheric cause which would diminish the velocity above or increase that below would produce the same effect, viz. would cause the waves to rise.

Such a cause must at certain times exist in the variation in the condition of the air as we proceed upwards from the surface.

Although barometric pressure does not affect the velocity of sound, yet, as is well known, the velocity of sound depends on the temperature*,

* It varies as the square root of $\frac{\text{pressure}}{\text{density}}$, and consequently as the square root of the absolute temperature.

and every degree of temperature between 32° and 70° adds approximately 1 foot per second to the velocity of sound. This velocity also increases with the quantity of moisture in the air; but the quantity is at all times too small to produce an appreciable result. This vapour nevertheless plays an important part in the phenomena under consideration; for it gives to the air a much greater power of radiating and absorbing heat, and thus renders it much more susceptible of changes in the action of the sun.

If, then, the air were all at the same temperature and equally saturated with moisture, the velocity of sound would be the same at all elevations; but if the temperature is greater, or if it contains more water below than above, then the wave of sound will proceed quicker below than above, and will be turned up in the same way as against a wind. This action of the atmosphere is, strictly speaking, analogous to the refraction of light. In light, however, it is density which retards motion; temperature and pressure have little or nothing to do with it; and since the density increases downwards, the rays of light move slower below than they do above, and are therefore bent downwards, and thus the distance at which we can see objects is increased. With sound, however, since it is temperature which affects the velocity, the reverse is the case; the rays are bent upwards, and the distance from which we can hear is reduced.

It is a well-known fact that the temperature of the air diminishes as we proceed upwards, and that it also contains less vapour. Hence it follows that, as a rule, the waves of sound must travel faster below than they do above, and thus be refracted or turned upward.

The variation of temperature is, however, by no means constant, and a little consideration serves to show that it will be greatest in a quiet atmosphere when the sun is shining. The sun's rays, acting most powerfully on that air which contains the most vapour, warms the lower strata more than those above them; and besides this, they warm the surface of the earth, and this warmth is taken up by the air in contact with it. It is not, however, only on such considerations as these that we are in a position to assert the law of variation of atmospheric temperature. Mr. Glaisher has furnished us with information on the subject which places it beyond the region of surmise.

I extract the following from his "Report on Eight Balloon Ascents in 1862" (Brit. Assoc. Rep. 1862, p. 462):—

"From these results the decline of temperature when the sky was cloudy

For the first 300 feet	was	0°·5	for every	100 feet.
From 300 to 3400	,,	0°·4	,,	,,
,, 3400 to 5000	,,	0°·3	,,	,,

"Therefore in cloudy states of the sky the temperature of the air decreased nearly uniformly with the height above the surface of the earth nearly up to the cloud.

"When the sky was partially cloudy the decline of temperature

In the first 100 feet was 0°·9

* * * *

From 2900 to 5000 „ 0°·3 for every 100 feet.

"The decline of temperature near the earth with a partially clear sky is nearly double that with a cloudy sky.

"In some cases, as on July 30th, the decline of temperature in the first 100 feet was as large as 1°·1."

We may say, therefore, that when the sky is clear the variation of temperature as we proceed upwards from 1 to 3000 feet will be more than double what it is when the sky is cloudy. And since for such small variations the variation in the velocity of sound, that is the refraction, is proportional to the temperature, this refraction will be twice as great with a clear sky as when the sky is cloudy.

This is the mean difference, and there are doubtless exceptional cases in which the variations are both greater and less than those given; during the night the variations are less than during the day, and again in winter than in summer.

This reasoning at once suggested an explanation of the well-known fact that sounds are less intense during the day than at night. This is a matter of common observation, and has been the subject of scientific inquiry. F. De La Roche discusses the subject, and exposes the fallacies of several theories advanced to account for it. Amongst others there are some remarks by Humboldt, in which he says that the difference is not due to the quietness of the night, for he had observed the same thing near the torrid zone, where the day seemed quieter than the night, which was rendered noisy with insects.

It is, however, by the experiments of Prof. Tyndall that this fact has been fully brought to light; and from their definite character they afford an opportunity of applying the explanation, and furnish a test of its soundness.

Neglecting the divergence of the bottom of the waves, a difference of 1 degree in the 100 feet would cause the rays of sound, otherwise horizontal, to move on a circle, the radius of which by the previous rule $=1100 \cdot \frac{100}{1}=110{,}000$ feet. A variation of one half this would cause them to move on a circle of 220,000 feet radius. From the radii of these circles we can calculate the range of the sound from different elevations.

With a clear sky, *i.e.* with a radius 110,000 feet from an elevation of 235 feet, the sound would be audible with full force to 1·36 mile; the direct sound would then be lifted above the surface, and only the diverging sound would be audible. From an elevation of 15 feet, however, the direct sound might be heard to a distance of ·36, or $\frac{1}{3}$ mile further, so that in all it could be heard 1·72 ($1\frac{3}{4}$) mile.

With a cloudy sky, *i.e.* with a radius 220,000 feet, the direct sound

would be heard to 2·4 miles from an elevation of 15 feet, or 1·4 times what it is with the clear sky. These results have been obtained by taking the extreme variations of temperature at the surface of the earth. At certain times, however, in the evening, or when it was raining, the variation would be much less than this, in which case the direct sound would be heard to much greater distances.

[So far I have only spoken of the direct or geometrical rays of sound, that is, I have supposed the edge of the sound to be definite, and not fringed with diverging rays; but, as has been already explained, the sound would diverge downwards, and from this cause would be heard to a considerable distance beyond the point at which the direct rays first left the ground. From this point, however, the sound would become rapidly fainter until it was lost. The extension which divergence would thus add to the range of the sound would obviously depend on the refraction—that is to say, when the direct rays were last refracted upwards, the extension of the range due to divergence would be greatest. It is difficult to say what the precise effect of this divergence would be; but we may assume that it would be similar to that which was found in the case of wind, only the refraction being so much smaller the extension of the range by divergence would be greater. On the whole the results calculated from the data furnished by Mr. Glaisher agree in a remarkable manner with those observed; for if we add ¼ mile for the extension of the range by divergence, the calculated distance with a clear sky would be two miles from a cliff 235 feet high.—*September* 1874.]

Now Prof. Tyndall found that from the cliffs at the South Foreland, 235 feet high, the minimum range of sound was a little more than 2 miles, and that this occurred on a quiet July day with hot sunshine. The ordinary range seemed to be from 3 to 5 miles when the weather was dull, although sometimes, particularly in the evening, the sounds were heard as far as 15 miles. This was, however, only under very exceptional circumstances. Prof. Tyndall also found that the interposition of a cloud was followed by an almost immediate extension of the range of the sound. I extract the following passages from Prof. Tyndall's Report:—

"On June 2 the maximum range, at first only 3 miles, afterwards ran up to about 6 miles.

"Optically, June 3 was not at all a promising day; the clouds were dark and threatening, and the air filled with a faint haze; nevertheless the horns were fairly audible at 9 miles. An exceedingly heavy rain-shower approached us at a galloping speed. The sound was not sensibly impaired during the continuance of the rain.

"July 3 was a lovely morning: the sky was of a stainless blue, the air calm, and the sea smooth. I thought we should be able to hear a long way off. We steamed beyond the pier end and listened. The steam-clouds were there, showing the whistles to be active; the smoke-puffs

were there, attesting the activity of the guns. Nothing was heard. We went nearer; but at two miles horns and whistles and guns were equally inaudible. This, however, being near the limit of the sound-shadow, I thought that might have something to do with the effect, so we steamed right in front of the station, and halted at $3\frac{3}{4}$ miles from it. Not a ripple nor a breath of air disturbed the stillness on board, but we heard nothing. There were the steam-puffs from the whistles, and we knew that between every two puffs the horn-sounds were embraced, but we heard nothing. We signalled for the guns; there were the smoke-puffs apparently close at hand, but not the slightest sound. It was mere dumb-show on the Foreland. We steamed in to 3 miles, halted, and listened with all attention. Neither the horns nor the whistles sent us the slightest hint of a sound. The guns were again signalled for; five of them were fired, some elevated, some fired point-blank at us. Not one of them was heard. We steamed in to two miles, and had the guns again fired: the howitzer and mortar with 3-lb. charges yielded the faintest thud, and the 18-pounder was quite unheard.

"In the presence of these facts I stood amazed and confounded; for it had been assumed and affirmed by distinguished men who had given special attention to this subject, that a clear, calm atmosphere was the best vehicle of sound: optical clearness and acoustic clearness were supposed to go hand in hand * * *.

"As I stood upon the deck of the 'Irené' pondering this question, I became conscious of the exceeding power of the sun beating against my back and heating the objects near me. Beams of equal power were falling on the sea, and must have produced copious evaporation. That the vapour generated should so rise and mingle with the air as to form an absolutely homogeneous mixture I considered in the highest degree improbable. It would be sure, I thought, to streak and mottle the atmosphere with spaces, in which the air would be in different degrees saturated, or it might be displaced by the vapour. At the limiting surfaces of these spaces, though invisible, we should have the conditions necessary to the production of partial echoes, and the consequent waste of sound.

"Curiously enough, the conditions necessary for the testing of this explanation immediately set in. At 3.15 P.M. a cloud threw itself athwart the sun, and shaded the entire space between us and the South Foreland. The production of vapour was checked by the interposition of this screen, that already in the air being at the same time allowed to mix with it more perfectly; hence the probability of improved transmission. To test this inference the steamer was turned and urged back to our last position of inaudibility. The sounds, as I expected, were distinctly though faintly heard. This was at 3 miles distance. At $3\frac{3}{4}$ miles we had the guns fired, both point-blank and elevated. The faintest thud was all that we heard; but we did hear a thud, whereas we had previously

heard nothing, either here or three quarters of a mile nearer. We steamed out to $4\frac{1}{4}$ miles, when the sounds were for a moment faintly heard, but they fell away as we waited; and though the greatest quietness reigned on board, and though the sea was without a ripple, we could hear nothing. We could plainly see the steam-puffs which announced the beginning and the end of a series of trumpet-blasts, but the blasts themselves were quite inaudible.

"It was now 4 P.M., and my intention at first was to halt at this distance, which was beyond the sound-range, but not far beyond it, and see whether the lowering of the sun would not restore the power of the atmosphere to transmit the sound. But after waiting a little, the anchoring of a boat was suggested; and though loth to lose the anticipated revival of the sounds myself, I agreed to this arrangement. Two men were placed in the boat, and requested to give all attention, so as to hear the sound if possible. With perfect stillness around them, they heard nothing. They were then instructed to hoist a signal if they should hear the sounds, and to keep it hoisted as long as the sounds continued.

"At 4.45 we quitted them and steamed towards the South Sand Head light-ship. Precisely fifteen minutes after we had separated from them the flag was hoisted. The sound, as anticipated, had at length succeeded in piercing the body of air between the boat and the shore.

"On returning to our anchored boat, we learned that when the flag was hoisted the horn-sounds were heard, that they were succeeded after a little time by the whistle-sounds, and that both increased in intensity as the evening advanced. On our arrival of course we heard the sounds ourselves.

"The conjectured explanation of the stoppage of the sounds appeared to be thus reduced to demonstration; but we pushed the proof still further by steaming further out. At $5\frac{3}{4}$ miles we halted and heard the sounds. At 6 miles we heard them distinctly, but so feebly that we thought we had reached the limit of the sound-range; but while we waited the sound rose in power. We steamed to the Varne buoy, which is $7\frac{3}{4}$ miles from the signal-station, and heard the sounds there better than at 6 miles distance.

"Steaming on to the Varne light-ship, which is situated at the other end of the Varne shoal, we hailed the master, and were informed by him that up to 5 P.M. nothing had been heard. At that hour the sounds began to be audible. He described one of them as 'very gross, resembling the bellowing of a bull,' which very accurately characterizes the sound of the large American steam-whistle. At the Varne light-ship, therefore, the sounds had been heard towards the close of the day, though it is $12\frac{3}{4}$ miles from the signal-station."

Here we see that the very conditions which actually diminished the range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and

recorded by Prof. Tyndall with his mind altogether unbiased with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so he would probably have ascended the mast, and thus found whether or not the sound was all the time passing over his head. On the worst day an ascent of 30 feet should have extended the range nearly ¼ mile.

The height of the sound-producing instruments is apparently treated as a subordinate question by Prof. Tyndall. At the commencement of his lecture, he stated that the instruments were mounted on the top and at the bottom of the cliff; and he subsequently speaks of their being 235 feet above him. He does not, however, take any notice of the comparative range of those on the top and those at the bottom of the cliff; but wherever he mentions them he speaks of them as on the cliff, leading me to suppose that for some reason those at the bottom of the cliff had been abandoned, or that they were less efficient than those above. If I am right in this surmise, if the sounds from below did not range so far as those from above, it is a fact in accordance with refraction, but of which, I think, Prof. Tyndall has offered no explanation.

[Besides the results of Prof. Tyndall's experiments there are many other phenomena which are explained by this refraction. Humboldt could hear the falls of Orinoco three times as loud by night as by day at a distance of one league; and he states that the same phenomenon has been observed near every waterfall in Europe. And although Humboldt gave another explanation*, which was very reasonable when applied to the particular case at Orinoco†, yet it must be admitted that the circumstances were such as would cause great upward refraction; and hence there can be but little doubt that refraction had a good deal to do with the diminution of the sound by day.

In fact if this refraction of sound exists, then, according to Mr. Glaisher's observations, it must be seldom that we can hear distant sounds with any thing like their full distinctness, particularly by day; and any elevation in the observer or the source of the sound above the

* "That the sun acts upon the propagation and intensity of sound by the obstacles met in currents of air of different density, and by the partial undulations of the atmosphere arising from unequal heating of different parts of the soil. . . . . . During the day there is a sudden interruption of density wherever small streamlets of air of a high temperature rise over parts of the soil unequally heated. The sonorous undulations are divided, as the rays of light are refracted wherever strata of air of unequal density are contiguous. The propagation of sound is altered when a stratum of hydrogen gas is made to rise over a stratum of atmospheric air in a tube closed at one end; and M. Biot has well explained, by the interposition of bubbles of carbonic acid gas, why a glass filled with champagne is not sonorous so long as that gas is evolved and passing through the strata of the liquid."—*Humboldt's Travels*, Bohn's Series, vol. ii. p. 264.

† The sounds proceeded over a plane covered with rank vegetation interspersed with black rocks. These latter attained a very considerable elevation of temperature under the effects of the tropical sun, as much as 48° C., while the air was only 28°; and hence over each rock there would be a column of hot air ascending.

intervening ground will increase this range and distinctness, as will also a gentle wind, which brings the sound down and so counteracts the effect of refraction. And hence we have an explanation of the surprising distances to which sounds can sometimes be heard, particularly the explosion of meteors, as well as a reason for the custom of elevating church-bells and sounds to be heard at great distances.—*September* 1874.]

INDEX TO VOL. XXII.

ABEL (F. A.), contributions to the history of explosive agents: second memoir, 160.
—— and Noble (Capt.), researches on explosives: fired gunpowder, 408.
Absorption phenomena, on a new class of, 378.
—— spectra of potassium and sodium at low temperatures, on the, 362.
Actinia, on the nervous system of: Part I., 44, 263.
Address of the President, 2.
Adiabatics and isothermals of water, on the, 451.
Agents, on explosive, 160.
Airy (H.) on leaf-arrangement, 298.
Aldehyde, formic, on the synthesis of, 171.
Alimentary canal, on the minute anatomy of the, 293.
Allman (Prof.), royal medal awarded to, 11.
Alps, on some winter thermometric observations in the, 317.
Anatomy of the genus *Phronima*, on the, 154.
—— of the lymphatic system of the lungs, contributions to the normal and pathological, 133.
Anniversary Meeting, Dec. 1, 1873, 1.
Atlantic, South, on dredgings and deep-sea soundings in the, 423.
Atmosphere, non-homogeneous, on the stoppage of sound by partial reflections in a, 190.
——, on the refraction of sound by the, 295, 531.
——, preliminary account of an investigation on the transmission of sound by the, 58.
Auditors, Report of, 1.

Bakerian Lecture announced, 297.
Barlow (W. H.) on the pneumatic action which accompanies the articulation of sounds by the human voice, as exhibited by a recording instrument, 277.
Beddoe (J.), admitted, 27.
Biogenesis, studies on, 289.
Blanford (W. T.), admitted, 362.
—— (H. F.), the winds of Northern India, in relation to the temperature and vapour-constituent of the atmosphere, 210.
Blood, on some points connected with the circulation of the, arrived at from a study of the sphygmograph-trace, 291.
—— corpuscles in Mammalia, on the intracellular development of, 243.
Bodily temperature, &c., of healthy men, on the influence of brandy on the, 172.
Brady (H. B.), admitted, 380.
Brain, on the localisation of function in the, 229.
Brandy, on the influence of, on the bodily temperature, the pulse, and the respirations of healthy men, 172.
Brodie (Sir B. C.) on the synthesis of formic aldehyde, 171.
Brom-iodides, on the, 51.
Broun (J. A.) on the annual variation of the magnetic declination, 254.
—— on the period of hemispheral excess of sun-spots, and the 26-day period, 43.
—— on the sun-spot period and the rainfall, 469.
Brunton (T. L.), admitted, 362.
—— and Fayrer (J.) on the nature and physiological action of the poison of *Naja tripudians* and other Indian venomous snakes; Part II., 68.
—— and Power (H.) on the diuretic action of *Digitalis*, 420.
Buchanan (J. Y.) on the absorption of carbonic acid by saline solutions, 192, 483.
——, some observations on sea-water ice, 431.

Calculus of factorials, on the, 434.
Candidates for election, list of, March 5, 1874, 228.
—— selected, list of, May 7, 1874, 310.

Carbonic acid, on the absorption of, by saline solutions, 192, 483.
Cardwell (Rt. Hon. E.), elected, 42; admitted, 241.
Cave-deposits of England, on the alleged existence of remains of a lemming in, 364.
Cayley (Prof.), a memoir on the transformation of elliptic functions, 56.
Centre of motion in the human eye, on the, 429.
Cerebral hemispheres, on the excitation of the surface of the, by induced currents, 368.
Chemical action of total daylight, on the, 158.
—— constitution of saline solutions, on the, 241.
Chinoline and pyridine bases, on the physiological action of the, 432.
Clifford (W. K.), admitted, 362.
Coal-measures, on the fossil plants of the, 248.
Colour, on the combinations of, by means of polarized light, 354.
Coniferine, on, and its conversion into the aromatic principle of vanilla, 398.
Connective tissue, nerve, and muscle, on the anatomy of, with special reference to their connexion with the lymphatic system, 380, 515.
Contributions to the history of the orcins: No. IV. On the iodo-derivatives of the orcins, 53.
—— to the history of explosive agents: second memoir, 160.
—— to the developmental history of the Mollusca: Sections I.–IV., 232.
—— to terrestrial magnetism: No. XIV., 461.
Copley medal awarded to H. L. F. Helmholtz, 10.
Copper wire, preliminary experiments on a magnetized, 311.
Cottrell (J.) on the division of a sound-wave by a layer of flame or heated gas into a reflected and a transmitted wave, 190.
Council, list of, 12.
Crookes (W.) on the action of heat on gravitating masses, 37.
Croonian Lecture announced, 297.

Daylight, total, on a self-recording method of measuring the intensity of the chemical action of, 158.
Dechevrens (M.), magnetic observations at Zi-Ka-Wei, 440.
Declination, on the annual variation of the magnetic, 254.
Deep-sea thermometer, on a new, 238.
Development, on the, of *Peripatus capensis*, 344.
Developmental history of the Mollusca, on the, 232.
Dewar (J.) and M'Kendrick (J. G.) on the physiological action of the chinoline and pyridine bases, 432.
Digitalis, on the diuretic action of, 420.
Displacement of the solar spectrum, note on, 219.
Diuretic action of *Digitalis*, on the, 420.
Donkin (A. E.) on an instrument for the composition of two harmonic curves, 196.
Double refraction, on, in a viscous fluid in motion, 46.
Dredgings and deep-sea soundings in the South Atlantic, on, 423.
Duncan (P. M.) on the nervous system of *Actinia*: Part I., 44, 263.

Earth, on the motions of some of the nebulæ towards or from the, 251.
Election of Fellows, 361.
Electric conductors, on the attractions of magnets and, 245.
Electrotorsion, on, 57.
Elliptic functions, on the transformation of, 56.
Energy, on volcanic, 328.
Experiments with safety-lamps, 441.
Explosive agents, contributions to the history of: second memoir, 160.
Explosives, researches on: fired gunpowder, 408.

Factorials, on the calculus of, 434.
Fayrer (J.) and Brunton (T. L.) on the nature and physiological action of the poison of *Naja tripudians* and other Indian venomous snakes: Part II., 68.
Fellows deceased, 1; elected, 2, 361; number of, 13.
Ferrier (D.), the localization of function in the brain, 229.
Financial statement, 14, 15.
Fireman's respirator, on some recent experiments with a, 359.
Forced breathing, on the bending of the ribs in, 42.
Forces, on the, caused by evaporation from, and condensation at, a surface, 401.
Formic aldehyde, on the synthesis of, 171.
Fossil plants of the coal-measures, on the organization of the: Part VI. Ferns, 248.
Frankland (E.) on some winter thermometric observations in the Alps, 317.
Franks (A. W.), admitted, 380.
Function in the brain, on the localization of, 229.

Galapagos Islands, on the tortoises of the, 421.

Galloway (W.), experiments with safety-lamps, 441.
Garrod (A. H.) on some points connected with the circulation of the blood, arrived at from a study of the sphygmograph-trace, 291.
Geological ages (or groups of formations), on the comparative value of certain, considered as items of geological time, 145, 334.
Gore (G.) on electrotorsion, 57.
—— on the attractions of magnets and electric conductors, 245.
Government Grant, account of appropriation of, 1873, 19.
Gravitating masses, on the action of heat on, 37.
Grubb (T.) on the improvement of the spectroscope, 308.
Gunpowder, on fired, 408.
Günther (A.), description of the living and extinct races of gigantic land-tortoises: Parts I. & II. Introduction, and the tortoises of the Galapagos Islands, 421.

Haarmann (W.) and Tiemann (F.) on coniferine, and its conversion into the aromatic principle of vanilla, 398.
Habits of the genus *Phronima*, on the, 154.
Harmonic curves, on an instrument for the composition of two, 196.
Hartley (W. N.), preliminary notice of experiments concerning the chemical constitution of saline solutions, 241.
Heat, on the action of, on gravitating masses, 37.
Helmholtz (H. L. F.), Copley medal awarded to, 10.
Hemispheres, cerebral, on the excitation of the surface of the, by induced currents, 368.
Hemispheral excess of sun-spots, on the period of, 43.
Hennessey (J. H. N.) on displacement of the solar spectrum, 219.
—— on white lines in the solar spectrum, 221.
—— on the periodicity of rainfall, 286.
Henrici (O.), admitted, 380.
Hewett (P. G.), admitted, 362.
Howard (J. E.), admitted, 362.
Huggins (W.) on the motions of some of the nebulæ towards or from the earth, 251.
Human eye, on the centre of motion in the, 429.
—— voice, on the pneumatic action which accompanies the articulation of sounds by the, as exhibited by a recording instrument, 277.

Ice, sea-water, some observations on, 431.
Imray (J.), the uniform wave of oscillation, 350.
India, on the winds of northern, in relation to the temperature and vapour-constituent of the atmosphere, 210.
Indian venomous snakes, on the poison of, 68.
Iodo-derivatives of the orcins, on the, 53.
Isothermals and adiabatics of water, on the, 451.

Kew Committee, report of the, 20.
Klein (E.), contributions to the normal and pathological anatomy of the lymphatic system of the lungs, 133.
—— on the smallpox of sheep, 388.

Land-tortoises, on the living and extinct races of: Parts I. & II. Introduction, and the tortoises of the Galapagos Islands, 421.
Lankester (E. R.), contributions to the developmental history of the Mollusca; Sections I., II., III., IV., 232.
Leaf-arrangement, on, 298.
Lemming, on the alleged existence of remains of a, in cave-deposits of England, 364.
Liquid fusion, on the alleged expansion in volume of various substances in passing by refrigeration from the state of, to that of solidification, 366.
Liquor sanguinis, on certain organisms occurring in the, 391.
Lockyer (J. N.), researches in spectrum-analysis in connexion with the spectrum of the sun: No. IV., 391.
——, spectroscopic notes: No. I. On the absorption of great thicknesses of metallic and metalloidal vapours, 371; No. II. On the evidence of variation in molecular structure, 372; No. III. On the molecular structure of vapours in connexion with their densities, 374; No. IV. On a new class of absorption phenomena, 378.
—— and Seabroke (G. M.), spectroscopic observations of the sun, 247.
Logan (H. F. C.) on the calculus of factorials, 434.
Lungs, contributions to the normal and pathological anatomy of the lymphatic system of the, 133.
Lymphatic system, on the anatomy of connective tissue, nerve, and muscle, with special reference to their connexion with the, 380, 515.

Macdonald (J. D.) on the anatomy and habits of the genus *Phronima* (Latr.), 154.

M'Kendrick (J. G.) and Dewar (J.) on the physiological action of the chinoline and pyridine bases, 432.
Magnetic declination, on the annual variation of the, 254.
—— observations at Zi-Ka-Wei, 440.
Magnetism, on the 26-day period of terrestrial, 43.
Magnetized copper wire, experiments on a, 311.
Magnets and electric conductors, on the attractions of, 245.
Maine (Sir H. S.), admitted, 380.
Male of *Thaumops pellucida*, on the, 42.
Mallet (R.), addition to the paper "Volcanic energy: an attempt to develop its true origin and cosmical relations," 328.
—— on the alleged expansion in volume of various substances in passing by refrigeration from the state of liquid fusion to that of solidification, 366.
—— on the mechanism of Stromboli, 473, 496.
Mammalia, on the intracellular development of blood-corpuscles in, 243.
Maxwell (J. C.) on double refraction in a viscous fluid in motion, 46.
Mechanism of Stromboli, on the, 473, 496.
Medals, presentation of the, 10.
Metallic and metalloidal vapours, on the absorption of great thicknesses of, 371.
Meteorological use of a planimeter, 435.
Mills (E. J.), admitted, 362.
Minute anatomy of the alimentary canal, on the, 293.
Molecular structure, on the evidence of variation in, 372.
—— —— of vapours, on the, in connexion with their densities, 374.
Mollusca, contributions to the developmental history of the, 232.
Moseley (H. N.) on the structure and development of *Peripatus capensis*, 344.
Mucous membrane of the uterus, on the structure of the, and its periodical changes, 297.

Naja tripudians and other Indian venomous snakes, on the nature and physiological action of the poison of, 68.
Nebulæ, on the motions of some of the, towards or from the earth, 251.
Negretti (H.) and Zambra (J. W.) on a new deep-sea thermometer, 238.
Nervous system of *Actinia*, on the, 44, 263.
Noble (Capt.) and Abel (F. A.), researches on explosives: fired gunpowder, 408.
Numerical value of π, on certain discrepancies in the published, 45.

Obituary notices of Fellows deceased:—
Archibald Smith, i.
Observations, spectroscopic, of the sun, 247.
Orcins, contributions to the history of the: No. IV. On the iodo-derivatives of the orcins, 53.
Organisms, on certain, occurring in the liquor sanguinis, 391.
Oscillation, on the uniform wave of, 350.
Osler (W.), an account of certain organisms occurring in the liquor sanguinis, 391.
Owen (R.) on the alleged existence of remains of a lemming in cave-deposits of England, 364.

Parkes (E. A.) on the influence of brandy on the bodily temperature, the pulse, and the respirations of healthy men, 172.
Periodicity of rainfall, on the, 286.
Peripatus capensis, on the structure and development of, 344.
Perry (S. J.), admitted, 362.
Phronima, on the anatomy and habits of the genus, 154.
Planimeter, on the employment of a, to obtain mean values from the traces of continuously self-recording meteorological instruments, 435.
Poison of *Naja tripudians* and other Indian venomous snakes, on the nature and physiological action of the, 68.
Polarized light, on the combinations of colour by means of, 354.
Potassium and sodium, on the absorption-spectra of, at low temperatures, 362.
Power (H.) and Brunton (T. L.) on the diuretic action of *Digitalis*, 420.
Presentation of the medals, 10.
Presents, list of, 48, 148, 223, 258, 329, 473.
President's Address, 2.
Prestwich (J.), tables of temperatures of the sea at various depths below the surface, taken between 1749 and 1868; collated and reduced, with notes and sections, 462.
Prime, on the number of figures in the reciprocal of every, between 20,000 and 30,000, 384.
—— number below 20,000, on the number of figures in the period of the reciprocal of every, 200.
—— ——, given the number of figures (not exceeding 100) in the reciprocal of a, to determine the prime itself, 381.
Pyridine and chinoline bases, on the physiological action of the, 432.

Rainfall, on the periodicity of, 286.

Rainfall, on the sun-spot period and the, 469.
Ramsay (A. C.) on the comparative value of certain geological ages (or groups of formations) considered as items of geological time, 145, 334.
Ransome (A.) on the bending of the ribs in forced breathing, 42.
Report of Auditors, 1.
—— of the Kew Committee, 20.
Researches in spectrum-analysis in connexion with the spectrum of the sun: No. IV., 391.
Respirator, on some recent experiments with a fireman's, 359.
Reynolds (O.) on the refraction of sound by the atmosphere, 295, 531.
——, on the forces caused by evaporation from, and condensation at, a surface, 401.
Ribs, on the bending of the, in forced breathing, 42.
Roberts (W.), studies on biogenesis, 289.
Roscoe (H. E.), Royal medal awarded to, 11.
—— on a self-recording method of measuring the intensity of the chemical action of total daylight, 158.
—— and Schuster (A.) on the absorption-spectra of potassium and sodium at low temperatures, 362.
Royal medal awarded to Prof. Allman and Prof. H. E. Roscoe, 11.
Rücker (A. W.) on the adiabatics and isothermals of water, 451.

Sabine (Sir E.), contributions to terrestrial magnetism: No. XIV., 461.
Safety-lamps, experiments with, 441.
Saline solutions, on the absorption of carbonic acid by, 192, 483.
—— ——, preliminary notice of experiments concerning the chemical constitution of, 241.
Salvin (O.), admitted, 380.
Sanderson (J. B.) on the excitation of the surface of the cerebral hemispheres by induced currents, 368.
Schäfer (E. A.) on the intracellular development of blood-corpuscles in Mammalia, 243.
Schuster (A.) and Roscoe (H. E.) on the absorption-spectra of potassium and sodium at low temperatures, 362.
—— and Stewart (B.), preliminary experiments on a magnetized copper wire, 311.
Scott (R. H.) on the employment of a planimeter to obtain mean values from the traces of continuously self-recording meteorological instruments, 435.
Sea, temperatures of the, at various depths below the surface, taken between 1749 and 1868; collated and reduced, with notes and sections, 462.
Sea-water ice, some observations on, 431.
Seabroke (G. M.) and Lockyer (J. N.), spectroscopic observations of the sun, 247.
Self-recording method, on a, of measuring the intensity of the chemical action of total daylight, 158.
Shanks (W.) on certain discrepancies in the published numerical value of π, 45.
—— on the number of figures in the period of the reciprocal of every prime number below 20,000, 200.
——, given the number of figures (not exceeding 100) in the reciprocal of a prime number, to determine the prime itself, 381.
—— on the number of figures in the reciprocal of every prime between 20,000 and 30,000, 384.
Sheep, on the small-pox of, 388.
Simpson (M.) on the brom-iodides, 51.
Smith (A.), obituary notice of, i.
Snakes, Indian venomous, on the poison of, 68.
Sodium and potassium, on the absorption-spectra of, at low temperatures, 362.
Solar spectrum, note on displacement of the, 219.
—— ——, on white lines in the, 221.
Solid states of water-substance, a quantitative investigation of certain relations between the gaseous, the liquid, and the, 27.
Sound, on the refraction of, by the atmosphere, 295, 531.
——, on the stoppage of, by partial reflections in a non-homogeneous atmosphere, 190.
——, on the transmission of, by the atmosphere, 58, 359.
—— wave, on the division of a, by a layer of flame or heated gas into a reflected and a transmitted wave, 190.
Sounds, on the pneumatic action which accompanies the articulation of, by the human voice, 277.
Spectroscope, on the improvement of the, 308.
Spectroscopic notes: No. I. On the absorption of great thicknesses of metallic and metalloidal vapours, 371; No. II. On the evidence of variation in molecular structure, 372; No. III. On the molecular structure of vapours in connexion with their densities, 374; No. IV. On a new class of absorption phenomena, 378.
Spectrum-analysis, researches in, in connexion with the spectrum of the sun: No. IV., 391.

Sphygmograph-trace, on some points connected with the circulation of the blood, arrived at from a study of the, 291.
Spottiswoode (W.) on combinations of colour by means of polarized light, 354.
Stenhouse (J.), contributions to the history of the orcins: No. IV. On the iododerivatives of the orcins, 53.
Stewart (B.) and Schuster (A.), preliminary experiments on a magnetized copper wire, 311.
Stromboli, on the mechanism of, 473, 496.
Structure and development of *Peripatus capensis*, on the, 344.
—— of *Thaumops pellucida*, on the, 42.
Studies on biogenesis, 289.
Substances, on the alleged expansion of various, on solidification, 366.
Sun, spectroscopic observations of the, 247.
—— spot period and the rainfall, on the, 469.
—— spots, on the period of hemispheral excess of, and the 26-day period of terrestrial magnetism, 43.
Surface-evaporation and condensation, on, 401.

Temperatures of the sea at various depths below the surface, 462.
Terrestrial magnetism, contributions to: No. XIV., 461.
—— ——, on the 26-day period of, 43.
Thaumops pellucida, on the male, and the structure of, 42.
Thermometer, on a new deep-sea, 238.
Thermometric observations in the Alps, on some winter, 317.
Thin (G.), a contribution to the anatomy of connective tissue, nerve, and muscle, with special reference to their connexion with the lymphatic system, 380, 515.
Thomson (J.), a quantitative investigation of certain relations between the gaseous, the liquid, and the solid states of water-substance, 27.
—— (W.) on dredgings and deep-sea soundings in the South Atlantic, 423.
Tiemann (F.) and Haarmann (W.) on coniferine, and its conversion into the aromatic principle of vanilla, 398.
Transformation of elliptic functions, on the, 56.
Trust funds, 16-18.
Tupper (J. L.) on the centre of motion in the human eye, 429.
Tyndall (J.), experimental demonstrations of the stoppage of sound by partial reflections in a non-homogeneous atmosphere, 190.
——, preliminary account of an investigation on the transmission of sound by the atmosphere, 58, 359.
—— on some recent experiments with a fireman's respirator, 359.

Uterus, on the structure of the mucous membrane of the, and its periodical changes, 297.

Vanilla, on coniferine and its conversion into the aromatic principle of, 398.
Vapours, metallic and metalloidal, on the absorption of great thicknesses of, 371.
Vice-Presidents appointed, 27.
Viscous fluid in motion, on double refraction in a, 46.
Volcanic energy: an attempt to develop its true origin and cosmical relations, 328.

Water, on the adiabatics and isothermals of, 451.
—— substance, a quantitative investigation of certain relations between the gaseous, the liquid, and the solid states of, 27.
Watney (H.) on the minute anatomy of the alimentary canal, 293.
Wave of oscillation, on the uniform, 350.
White lines in the solar spectrum, on, 221.
Willemöes-Suhm (R. von) on the male and the structure of *Thaumops pellucida*, 42.
Williams (J.) on the structure of the mucous membrane of the uterus and its periodical changes, 297.
Williamson (W. C.) on the organization of the fossil plants of the coal-measures: Part VI. Ferns, 248.
Wilson (C. W.), admitted, 362.
Winds of Northern India, on the, 210.

Zambra (J. W.) and Negretti (H.) on a new deep-sea thermometer, 238.
Zi-Ka-Wei, magnetic observations at, 440.

END OF THE TWENTY-SECOND VOLUME.

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